CURRENT TRENDS IN BRITISH PSYCHOLOGY

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PREFACE

THE year 1951 was the occasion for holding a Festival of Britain, and to this festival a distinctive contribution was made by the British Association for the Advancement of Science. At the Edinburgh meeting of the Association, prominence was given in all sections to the British contributions to the progress of science. Most sections included in their programmes historical surveys either of their science as a whole or of special fields of research. The Psychology Section decided to celebrate the occasion not so much by historical surveys as by presenting the position of British psychology to-day and its current trends.

The Presidential Address to the Section, on *Psychology and the Laity*,[1] reviewed the development of psychology in this country through the convergence of two main streams of reflection and research—the philosophical tradition and the studies of the British naturalists. From this convergence had come first the science and then the profession of psychology. The main theme of this address was that the health and the effectiveness both of the science and the profession depended upon the understanding co-operation of an educated laity.

The other papers presented to the section review or illustrate current trends in the theoretical science and its practical applications—social, industrial, educational and clinical. Most of these papers are here collected. They fall into two groups. Part I of this volume consists of surveys and studies illustrating the applications of psychology to practical problems. Part II contains papers concerned with conceptual analysis, methodology, and with the more theoretical research by which these applica-

[1] Published in the collected Presidential Addresses: *The Advancement of Science*, viii, No. 30.

tions are sustained. The authors represented in both parts were asked, first, to write in as non-technical a manner as their subjects permitted, and, secondly, to stress British contributions to these subjects. They have certainly tried to meet these requests, though it is obvious that in many branches the dominant contributions at the moment mostly come from across the Atlantic.

The papers which are not reproduced here were, in general, either of a more specialized nature or formed an integral part of joint meetings with other Sections. In the Physics Section a symposium was arranged on *Life and Work in Extreme Environmental Conditions.* To this a paper was contributed by Dr. N. H. Mackworth (Applied Psychology Unit, Cambridge University) on *Performance Studies of Severely Chilled Hands.* To the same Section a paper was read by Dr. R. Fürth (Birkbeck College, London University) on the *Physics of Social Equilibrium.* At a joint meeting of the Psychology and Zoology Sections, Professor R. W. Russell (University College, London University) contributed a paper on *Objective Studies of Abnormal Behaviour in Animals.* The paper by Professor Drew and Mr. George contained in the present volume was also read at this joint meeting. The more specialized meetings of the Psychology Section included papers by Dr. R. W. Pickford (Glasgow University) on *Further Work on Individual Differences in Colour Vision,* by C. A. Oakley (Glasgow) on *Psychology and the Prevention of Industrial Accidents,* and by Miss M. M. Lawlor and Miss S. B. N. Shimmin (Bedford College, London University) on *Belief in a Standard of Judgment and its Influence on Personal Preference,* and *Attitudes towards Personal Ambition.*

The studies presented here do not, of course, cover every branch of psychology in which British workers are interested; but we would claim that they are sufficiently representative to provide a fair picture of the condition of psychology in Britain in the mid-twentieth century.

C. A. M.

December 1951 P. E. V.

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PART ONE

FIELDS OF APPLIED PSYCHOLOGY

I

FIELD RESEARCH IN INDUSTRIAL PSYCHOLOGY

C. B. FRISBY

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THE DEVELOPMENT OF INDUSTRIAL PSYCHOLOGY

THE questions with which this paper deals, questions which, it is contended, are of particular importance to industrial psychology at the present time, may best be considered if the development of the subject in Great Britain in the last thirty years is kept in mind.

The work of the Health of Munition Worker's Committee from 1915 to 1918 had drawn attention to the relationship between human effectiveness at work, as measured by output, and matters such as shifts and hours. In the early 1920s the industrial psychologist's attention was particularly directed therefore, to the lines of approach already proved fruitful by that Committee, and to problems in which the methods of the psychological laboratory could be applied. Industrial psychology was, in fact, regarded then as a form of applied pyschology rather than as a branch of psychology which is the view now gaining acceptance in this country.

The first stage of the development of the subject was thus devoted to the study of *how* men work and how they are affected by factors in the physical environment. Movement study principles were applied with success to simplify working methods

so as to avoid the waste of mental and physical energy, and to reduce fatigue. The physical environment was studied primarily from the physiological point of view.

The second stage came with the application of methods for studying individual differences to the practice of vocational guidance and vocational selection. The particular contribution of the psychologist was first seen to lie in the provision of means for the more accurate assessment of general intelligence and special aptitudes by means of tests. Qualities of disposition were not ignored, but in the industrial field they tended to be thought of primarily from the point of view of the task situation —what demands did the job make in terms of such temperamental qualities as persistence or resistance to monotony. We are more cautious now in the use of such terms. Not very much was heard of the importance of assessing the individual's suitability as a member of the working group or of the likelihood of his getting on with his foreman. In a word, then, industrial psychology in its early years was primarily concerned with the physiological and cognitive, rather than with the orectic, aspects of work.

This, surely, is the reason why the Hawthorne experiments created so much interest. Elton Mayo and his collaborators demonstrated that, at least in certain circumstances, the physical conditions in which work was performed had less influence on output than the intangible factors associated with motivation and satisfaction in work. During the thirties increasing attention was paid to more purely psychological aspects of work. Wyatt's studies of repetitive work and payment methods were noteworthy in this respect. It was at this stage, too, that the National Institute of Industrial Psychology began to develop a method of studying the attitudes of workers to their jobs and to the company which employed them. But it was in the forties that the changed conditions in the industrial field led to the emergence of motivational problems as those most urgently needing solution.

INCENTIVES IN INDUSTRY

Between the wars a fairly considerable degree of unemployment had appeared to be the normal situation. Most men were glad to have a job. If a man gave up his work there were plenty more anxious to take it on. The economic factor and fear of unemployment seemed a sufficient explanation of why men worked; though it was recognized, of course, that for some, security of employment was more important than opportunity to obtain large earnings. It is true that the existence was admitted of people, said to have "vocation", who chose and apparently derived satisfaction from work in which material rewards were small, but which involved essential services to the community. And there was the artist—to the practical man an incomprehensible person prepared in the last resort to endure squalor and semi-starvation in order to pursue his chosen line of activity.

During the war, and after it, however, employers discovered that the economic factor was, after all, not the only one operating in industry and that many workers were not prepared to go on working harder and longer merely to increase their earnings. A classic example is probably the coal miner, who has shown himself usually willing to buy leisure when opportunity offers. The study of people in their working environment has consequently expanded as the industrial psychologist has attempted to deal with motivational aspects of work, with which before the war he had not been very greatly concerned. In the present industrial situation when an immediate increase in individual productivity offers the only solution to the nation's economic problem, these questions are even more important than those relating to the physical environment and methods of work.

What may be termed "social problems of work" have also attracted much attention. The relation of people within the working group, relationships between groups, between management and employed, have urgently demanded study. Here

again, physiological and cognitive factors are of little account by comparison with orectic ones.

Industry's own recognition of this kind of problem has led it to attach particular importance to certain questions recognized as having a great bearing on human relations in industry—to the training of supervisors for example. It has also led to the development of techniques, such as formal joint consultation, designed to improve relations between management and workers. Consequently, the industrial psychologist has been called upon to study management techniques in the human relations field, to attempt to evaluate and to improve them.

As a result, new problems of methodology have arisen; for example, the study of motivation and of relationships makes it essential to study the attitudes of people towards their working life or towards certain aspects of it. Sources of satisfaction have to be studied. Group organization and conventional behaviour patterns have to be considered. Three inquiries, sponsored by the Human Factors Panel of the Committee on Industrial Productivity, and begun by the National Institute of Industrial Psychology in 1948, provide illustrations of these problems.

AN INVESTIGATION OF JOINT CONSULTATION

One inquiry was concerned with joint consultation in British industry; it was a study of current practice in joint consultation and was an attempt to identify factors which appeared to be associated with success. The problems of sampling involved in an inquiry of this sort are extremely important and difficult, but there is no room for a discussion of them in this paper. The collection of relatively objective information from a group of companies about matters such as the number employed, the products, the joint consultation procedures, and so on was comparatively straightforward. But quite obviously the inquiry could not limit itself to material factors such as these. The success of joint consultation must be affected by the attitudes

of people at all levels in the organization to the idea of joint consultation. Their approach to it will be markedly affected by their views on the part that joint consultation has to play in an industrial organization and on what may be expected of it. The investigators were therefore immediately faced with the problem of assessing attitudes and value systems before they could complete an account of the conditions governing the operation of joint consultation in a firm. Furthermore, since joint consultation represents an attempt to provide a channel of communication in addition to that provided by the normal hierarchical structure of the organization, its success must be affected by relations within the hierarchy. Thus, the study of relationships was also involved.

Lastly, since it was hoped to identify factors associated with success, the definition of success in joint consultation and the means of assessing it had to be dealt with. A basic measure of success presents great difficulties, since different people in different firms have different ideas of what joint consultation might achieve; what would be success to one would be relative failure to another. After a number of preliminary studies of conditions in different firms, the investigators finally decided that success in joint consultation could best be defined as the extent to which joint consultation had assisted in the solution of problems within the firm. Potential problems were identified under thirty-one heads which fell into four categories, namely, individual adjustment, relationships, realization of a common purpose, and production. Objective evidence could be obtained very rarely indeed.

The method followed was to gather evidence from individual interviews with people from all levels within the organization about conditions under each heading prior to the introduction of joint consultation, and about changes since that time. The current situation was assessed and the causes for any changes discussed. An opinion had also to be formed from the interviews as to whether the factory had, or had had, a problem under each

head; and if so, how serious the problem was. Next, it had to be ascertained whether the problem had been discussed through the joint consultative procedure, or whether, if it had not been discussed, this was due to its exclusion by the terms of reference of the committee. These, at least, were concrete questions which could usually be settled by reference to the minutes and the constitution. The minutes of the consultative committees frequently provided useful evidence about the way in which a problem had been treated and the results achieved. Taking into account information from the minutes and the views of all those with whom the question was discussed, the investigator had to arrive at an assessment of the degree of effectiveness of the joint consultative procedures, an assessment under each of the separate thirty-one heads.

No one would pretend, of course, that such a method could yield results of a high degree of accuracy, but it illustrates the kind of difficulties in which the investigator must find himself when he starts to attempt to assess attitudes, relationships and the effects of procedures or changes on the life of an organization.

In this particular inquiry comparisons of the assessments of success made by the four investigators in the team yielded satisfactory results. The investigators had done their best to achieve a common standard by discussion of case studies and by making occasional visits to firms in pairs. Assessments of success were made in 102 firms in all. In the result, after making allowance for seventeen of them which were known to have particularly high standards of joint consultation, and checking that the distribution of the remaining eighty-five firms in respect of size, industry and region among the four members of the team showed no significant bias, a comparison was made of the mean assessments of the various investigators. The difference in mean assessment between any two of them was found to be not significant. Thus, some evidence could be offered that the investigators were producing answers which would have been

the same had another member of the team carried out the visit, but there is no evidence, of course, that the answers were the right ones.

A STUDY OF SUPERVISORS

A second inquiry was designed to survey current practices in industry concerning the status, selection and training of supervisors. A further aim was to seek to develop improved methods of selection and training and to evaluate them.

In this inquiry, as in the case of that on joint consultation, no great difficulties were encountered in collecting relatively objective information from managers about their company's practice on matters affecting the supervisors. What was more difficult was to carry out the job studies of supervisory work which were clearly necessary if consideration of questions of status, selection and training was to be soundly based. Here again, much reliance had to be placed on the interview as the main method of collecting information. Interviews with managers provided information about the roles which the manager considered his supervisors to be performing. In this way the supervisor's place in the hierarchy of each firm visited was assessed, and corroborative evidence was sought from the supervisor himself and from the evidence provided by the material aspects of status, such as pay and privileges. Interviews with the supervisors, besides providing information about day to day duties and responsibilities, threw light on each man's own views on his role, and were also the means of assessing attitudes towards the job.

Some criterion of supervisory effectiveness is clearly essential if a comparison is to be made between the relative values or different methods of selection and of training. There is, unfortunately, no progess to report on this question as yet; the effectiveness of a supervisor is particularly difficult to assess in a manner having any claim to objectivity.

RESEARCH ON THE UNIT OF WORK

A third research begun by the Institute in 1948 was concerned with repetitive work. The aim was to look for principles which govern the determination of the optimum unit of work in jobs which have been greatly subdivided and broken down with the development of modern production methods. It is clear that at some point in the breaking down process, for a variety of reasons, the operative will become less effective than he was with the larger task. The problem was how to give some guidance to the production engineer in identifying this point. A secondary question was that of the size of the batch of work issued to an operative at any one time. Previous investigations had suggested that there might be a relationship between rate of working and size of batch, possibly accounted for by the hypothesis that the batch presents a target to the worker and that the achievement of such a target should not be too remote.

The approach adopted was the classical experimental one—comparing the results of two or more different degrees of breakdown or different sizes of batch, the criteria available being output and the opinion of the workers. The workers were taken into the confidence of the investigators, and in the course of interviews each was told something of the purpose of the inquiry, and his general attitude towards his job was explored. Once more, interviewing for the assessment of attitudes was highly important. It was possible to carry out only a few such experiments, but in addition studies were made of several jobs. These studies, and the results of interviews with workers, threw a good deal of light on the extraordinary complexity of factors affecting the worker's attitude to the actual task, and it was decided that a more fundamental attack on the problems of repetitive work was called for. Consequently, a series of job studies was begun in order to explore the sources of satisfaction and dissatisfaction which the operative finds in his or her work. These studies are still in progress; the investigators not only observe the task and

carry it out for themselves wherever possible, but they discuss it with supervisors, trainers and operatives, and again the interview has emerged as the most important tool of those engaged on this research. Through it they have to seek to identify sources of satisfaction or dissatisfaction and to evaluate them against the general picture of the worker's personality and standard of values.

CONCLUSIONS

All three researches illustrate the importance of assessments of attitudes to work, of satisfaction derived from the working situation, and of motivational aspects of work. A basic methodological problem at present is consequently that of reliable means of assessing such factors in a way which will be not too time consuming and which will reduce to the minimum interruption of the workers' ordinary tasks. This latter point is of practical importance, since the co-operation which can be expected from both worker and employer is related to it.

The necessary reliance on the interview for the collection of so much information emphasizes the importance of interviewing skill for the industrial psychologist to-day. It stresses the need for more research on interviewing methods, and their reliability. Attitude scales in industrial work have so far been but little employed; their possibilities need to be further explored. The assessment of attitudes through the interview, free, or controlled to different degrees, has been studied recently by the Industrial Psychology Unit of the Medical Research Council, and it is hoped that a report will be available before long on its findings. But the staff of that Unit would probably be the first to agree that a great deal of work still remains to be done in this field.

Finally, all three inquiries have raised problems of criteria; an approach similar to that adopted in the joint consultation study might be useful when attempts are made to evaluate other management practices, but a good deal of research would be

necessary to determine its validity. The assessment of supervisory effectiveness is a particular case of the general problem of criteria by which to evaluate experiments in industrial psychology; the question of criteria is surely the most difficult, and perhaps the most important, of all those facing the industrial psychologist at the present time.

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II

VOCATIONAL GUIDANCE IN BRITAIN

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PREVIOUS WORK BY BRITISH PSYCHOLOGISTS

In reviewing the past, we cannot do better than start with the early contributions of Cyril Burt, who in 1922 was chosen by C. S. Myers to direct the vocational guidance activities of the newly incorporated National Institute of Industrial Psychology. After making a study of the posts actually obtained by 2,000 boys and girls leaving London elementary schools at the age of fourteen, Burt arranged for another 100 to be given advice by the National Institute. Information was gathered about them from their parents, teachers and school doctors; and they were tested and interviewed. The team of four investigators who saw all the young people made agreed recommendations for them, and at the end of two years a follow-up enquiry was conducted. The criteria used were three: the young worker's expression of satisfaction or dissatisfaction with his employment; his weekly earnings; and the length of time he had held his jobs. It was found that, by each of these criteria, the boys and girls who were in employment judged suitable by the investigators were more successful than those who were not, though the difference between the groups was significant only in regard to the first of these criteria. A report on the investigation was published by the then Industrial Fatigue Research Board, which

had already produced a useful survey, by Bernard Muscio, of the existing literature of vocational guidance.

Burt considered that the main shortcomings of his enquiry were three: first, the inadequacy of the original job-analyses; secondly, the unsatisfactoriness of the procedures adopted in recording facts and opinions about the young people advised; and thirdly, the absence of a control group. So in what became known as the second London experiment, also undertaken by the National Institute, carried out in the middle twenties and directed by F. M. Earle, an attempt was made to obtain profit from the earlier experience. The job analyses were more thorough; the documentation was more systematic; and experimental and control groups, each composed of 600 young people of both sexes, were assembled. To the three criteria of occupational success previously used, a fourth was added—an expression of satisfaction or dissatisfaction by the employer. The means adopted for collecting relevant data about the boys and girls were essentially the same as before: noteworthy differences appeared only in the testing scheme and in the medical examination arrangements. The findings of the enquiry, which confirmed broadly those of the first investigation, were published in a book called *Methods of Choosing a Career*, by Earle and others.

Next, in the later twenties, came the Birmingham experiment, which was very similar to the second London experiment in its procedures, criteria and conclusions. Here, however, an attempt was made to achieve further progress by a study of the extent to which it might be possible to use the National Institute's techniques within the framework of an official vocational guidance system operated by men and women who made no claim to be psychologists. In a paper read at the 1950 Annual Meeting of the British Association, Percival Smith dealt with this and certain allied Birmingham investigations, so it would be inappropriate for me to make more than passing reference to it. It is perhaps enough that I should stress the fact that the findings were consistent with the hypothesis that the superiority of the results

achieved with the experimental group were due to the superiority of the methods used in advising the group, and not, as a critic of the London investigations might have suggested, to the greater general competence of the people who employed those methods.

Then, in the early thirties, came the Borstal experiment, which I have myself reported in a publication of the Industrial Health Research Board. Four hundred male delinquents, between the ages of sixteen and twenty-one, were tested and interviewed at Wormwood Scrubs Boys' Prison, the London collecting-centre for the various Borstal institutions. Recommendations were formulated about the type of occupational training each youth should receive during his detention; but in half the cases the recommendations were not disclosed to the training authorities, who allocated the members of this control group to their work-parties by a customary method. For several reasons, the only usable criterion for judging the success of each youth was an expression of satisfaction or dissatisfaction made jointly by his housemaster and his instructor. On this basis everyone was graded A or B, successful or less successful, at the end of his detention, which ranged in length from a few months to about two years. Of the control group, who were put into their work-parties by the old method, 45·6 per cent became grade A workers. The corresponding percentage for the experimental group was 69·5; and the difference was significant. A feature of this experiment was its attempt not merely to determine the relative merits of two allocation procedures, considered as wholes, but also to investigate the predictive value of certain *parts* of the new procedure—the various objective tests employed.

Although there were, at about this time, other vocational guidance investigations conducted—or at least started—in Fife, Edinburgh, Rugby and Cambridge, I think it is true to say that the enquiries to which I have already referred are the ones which, in pre-war days, contributed most to the improvement of vocational guidance arrangements in Britain. But it must be

admitted that their influence was not, in fact, very great. It is true that the Birmingham work led to the adoption of a scheme, operated by the National Institute and the Birmingham Education Department, that affected schools all over that city and involved the training of many teachers and youth employment officers in modern vocational guidance methods. It is true that the Borstal work produced a series of courses in which selected Borstal housemasters were trained in testing and interviewing. It is true also that the experience gained in all these early investigations was of value to the National Institute in the planning and conduct of many courses for teachers, youth employment officers and others in London, Hull and Edinburgh particularly. But with the outbreak of war in 1939, or shortly afterwards, all these NIIP activities came to a full stop. And that meant, virtually, the suspension of almost all vocational guidance work by British psychologists; for although there were psychologists not directly connected with the National Institute who were engaged on either research or development in this field, they were very few indeed. The NIIP lost all its vocational guidance staff to the Royal Navy and the Army, with the single exception of Miss M. B. Stott.

But it would be wrong to assume that nothing more happened until the din of battle had died away. The three fighting services tried to tackle some of the vocational guidance problems of men and women due for release. The Royal Air Force set up an extensive Vocational Advice Service; and the Army developed a scheme that later provided useful experience for the Ministry of Labour. In the Royal Navy we had to content ourselves with a small-scale attempt to stimulate vocational self-guidance: sailors tend to be less accessible than soldiers and airmen, and it would not have been easy for us to produce a scheme whereby they visited some well-placed office or received the attention of a peripatetic advisory team. However, these arrangements, though perhaps interesting to those of us who are concerned with the problem of making our psychological techniques both

administratively convenient and politically defensible, were of no great importance scientifically.

What mattered very much more was the work of the Army's Civil Resettlement Units (CRUs), which aimed at facilitating the adjustment of returned prisoners of war to civil life. This work has been described and discussed by Curle and Trist in two rather neglected papers in *Human Relations*. "The function of these units", Curle tells us in the first of them, "was to act as bridges between the Army and civilian life, by providing a 'community' in which there was considerable inter-communication between these two spheres. This community was 'designed' along specifically psychological lines as a transitional society with the therapeutic aim of releasing those tensions which appeared to result primarily from POW experience, and of changing those attitudes which retarded the reassumption by the repatriate of a fully participant role in civilian life." It is scarcely surprising that vocational guidance found a place in the activities of the CRUs, which in 1946 numbered twenty and took up to sixty men a week each for courses lasting several weeks. A vocational officer and sergeant tester ran, for those who wanted it, a programme of testing, interviewing and group discussion. Socio-drama was used, and "job rehearsals" were arranged to allow men to try out various types of employment. A Ministry of Labour officer concerned with placement was resident in the CRU and took part in several of its ordinary activities. Fifteen criteria of social participation were used in a small follow-up enquiry carried out in one area, in which fifty CRU POWs, a hundred matched non-CRU POWs and a control group of forty civilians were studied. The statistical treatment of the data was simple but adequate; and the conclusions showed clearly, as one might expect, the interdependence of occupational satisfaction and satisfactions of other kinds.

THE PRESENT SITUATION

Let us look at the National Institute of Industrial Psychology, at the Ministry of Labour, at one or two other organizations, and at the university departments of psychology and education. The National Institute has not yet swung back into its stride in the field of vocational guidance, though it is now making substantial progress in the right direction. There is still a good deal to be done in the rebuilding of its "private practice", which was the foundation on which most of its scientific and technical work had to be constructed. There is scope for further development in the courses of instruction provided for teachers and youth employment officers—courses which reached a promising level of efficiency in the work done after the war for the local education authorities in Warrington and Preston. But a particularly pressing need is for more research; and the National Institute's realization of this is clearly indicated in a recent paper, in *Psychology at Work*, by Lock, the present head of its vocational guidance department. The fact that the vocational guidance procedures used and advocated by the NIIP are not substantially different from those described by Macrae in a pamphlet published many years ago—*The Case of John Jones*—is no doubt indicative of the essential soundness of the early work; but advance is now due.

In the Ministry of Labour orbit are to be found two developments of considerable interest to psychologists—in the Youth Employment Service and in the Industrial Rehabilitation Units. The Youth Employment Service is controlled by a body called the Central Youth Employment Executive, which is staffed by officers of the Ministries of Labour and Education, and of the Scottish Education Department; but it is operated in most places by the local education authority. For a number of reasons, some of which I have given recently in a *Current Affairs* pamphlet, we cannot reasonably think of a Youth Employment Service staffed by psychologists. We can, however, think of a Service

staffed by men and women who receive some of their training, and much of their day-to-day technical advice, from psychologists. This is, in fact, the goal towards which the Central Executive are working, not only through an inspectorate now headed by Gordon Whiting, formerly of the National Institute, but also through courses, conferences and appropriate literature. The main points of the present psychological attack are the interview, job analysis, and school reports. Of course, to a large extent this is, for the psychologist, merely development work; but it will undoubtedly throw up a good many research problems, some of them suitable for investigation by university departments and the independent institutes.

The Ministry's own Industrial Rehabilitation Units (I.R.U.s) now number fifteen. The work of one of them was outlined by Cavanagh in a note prepared for the 1949 Annual Meeting of the British Association and later published in the *Quarterly Bulletin of the British Psychological Society*. In general, the structure and function of these units resemble those of the Civil Resettlement Units I have mentioned, though in fact the first IRU was established before the CRUs came into existence. Their aim is to provide a "cushion" for men and women, of all ages from under twenty to over sixty, who are due for a return to employment after injury or sickness or long unemployment. Each unit has workshops, a garden and a gymnasium; and the team that enables the rehabilitees to make appropriate use of them is composed of a rehabilitation officer (in charge), a medical officer (usually part-time), a vocational officer, a social worker, a disablement resettlement officer (concerned with placement), and several occupational supervisors. In most cases the vocational officer, who sees all newcomers to the unit, is a psychologists His first job is to give tests and interviews and to collect report. from his colleagues: his second is to formulate advice and present it for discussion at a case conference. The follow-up procedures used at present are primitive; but the Edinburgh IRU is trying to tackle this important problem with the aid of a control

group, and other attempts to deal with it will soon be made elsewhere. A calculation based on figures given in the *Ministry of Labour Gazette* for 1951 (lix, 7) indicates that between six and seven thousand men and women are passing through these units in the course of a year.

Similar work is being carried out elsewhere, particularly at Roffey Park, an independent rehabilitation centre in Sussex, and at the Belmont Hospital, in Surrey. I understand that reports on their activities, of special interest to psychologists concerned with vocational guidance, will appear later. In these two institutions the emphasis is on a medical approach to rehabilitation: in the case of the Ministry of Labour units it is on what has been called an industrial approach. The distinction is sometimes difficult to make, but I think it is a sensible one. At this point it is convenient to take note of another relevant enquiry, recently carried out by O'Connor and Tizard, of the Maudsley Hospital, London, on the employability of certified mental defectives. This is reported in a paper published in *Occupational Psychology*.

In the university departments of psychology and education, where we might expect to find considerable vocational guidance activity, there is very little to be discovered. It is true that from time to time higher degree students present theses on vocational guidance topics. It is true also that vocational guidance is sometimes given in child guidance clinics and centres associated with such departments. But the volume of the work seems small; and its quality is rarely striking. However, there are signs of considerable life here and there, not least in the Edinburgh Applied Psychology Unit, whose purposes and procedures have been described with characteristic vigour by McMahon in another paper published in *Occupational Psychology*. In Liverpool, Hearnshaw has proceeded far with realistic plans for an occupational clinic which will, I imagine, have important training and research functions to perform. In London, at Birkbeck College, where in conjunction with the London School of Hygiene we provide a one-year postgraduate diploma

course in occupational psychology, we have started on a small scale a vocational guidance centre for the training of psychologists and the provision of research facilities. Our links with the Central Youth Employment Executive and the Ministry of Labour Industrial Rehabilitation Units are close, and seem likely to help us on both sides of our work. Our aim is not to give vocational guidance to large numbers of people, but to provide a sort of workshop, to which psychologists can come to learn about it by tackling problems that are both real and important.

FUTURE DEVELOPMENTS

In recent papers, Lock and McMahon and I have discussed current needs in vocational guidance research. There appears to be little disagreement between us; and in the circumstances I hope you will forgive me if I present here a brief restatement of what I have said already, with some elaboration. Studies of criteria of occupational success should, I consider, have pride of place in our research programmes. Without them, we can make little headway either in the validation of our vocational guidance procedures or in our understanding of the reasons why one procedure appears to be better than another. The criteria used in the early investigations to which I have referred—the satisfaction of the worker with his work, his satisfactoriness to his employer, his earnings, his stability of employment—have all proved worthwhile in a rough way: now they need to be broken down and supplemented. Linked to such studies we must have studies of occupational requirements, designed primarily to improve our methods of job-analysis by taking us away from the mere listing of necessary and desirable "qualities" based on bright hunches of an often unverifiable sort. In my opinion, a far more profitable approach was that used by the NIIP in pre-war personnel selection work for an air transport organization, whereby attention was concentrated on the

identification and description, in operational terms, of the causes of failure in the occupation under review. This line of attack, which I have outlined elsewhere, gains support from a similar but better-developed approach used during the war by American psychologists concerned with personnel selection and described by Flanagan in *Current Trends in Industrial Psychology*. It is called the method of critical requirements; and, as Flanagan remarks, "the first objective of the method . . . is to eliminate from further consideration all job requirements which are not critical to success or failure on the job". But in addition to making attempts to find or create better criteria of occupational success, and hammering out better methods of studying occupational requirements, we must tackle the problem of what I have called "grouping and grading" the occupations into which we are steering people. Our existing ways of classifying occupations are on the whole embarrassingly primitive.

These, then, are the research problems—or, at least, the fundamental research problems—that I am inclined to suggest for immediate consideration. But I must, in concluding this survey, confess that I am doubtful whether we shall make much real progress with them until we have more psychologists available who have had training and experience in ordinary day-to-day vocational guidance work, and who have themselves suffered the pains produced by our present lack of knowledge and skill. That is why I believe that what we should really do next is to produce more psychologists who can learn what vocational guidance is all about by doing it.

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III

APPLICATIONS OF PSYCHOLOGY IN THE DEFENCE DEPARTMENTS[1]

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INTRODUCTION

THE most beneficial of all military applications of psychology would be to prevent war, or at least greatly to diminish the risk of its occurrence. In time, this may not seem an impossible programme. Careful investigation of the psychological causes of war, followed by steps to induce modifications of social behaviour which would diminish the risk of their operating, are clearly desirable and may come before long. The more sanguine will see useful beginnings in such studies as the "Tensions Project" of UNESCO and in such work as that of the World Federation for Mental Health. But while defence psychologists of 1951 can envisage these developments it may well be 2051 before their successful completion is reported upon.

Failing the elimination of war, psychologists might be expected to make considerable contributions to so-called psychological warfare. This kind of warfare is highly important, for it aims at winning wars without actually fighting, or at least with a minimum of fighting and consequent destruction. However, the bulk of useful work in psychological warfare seems to have been done by non-psychologists—by journalists, advertising

[1] This chapter is published with the permission of the Admiralty, but the responsibility for all statements contained therein is the author's.

men, diplomats and arts dons. A possible exception is provided by some psychiatrists who have shown a considerable *flair*. The work of H. V. Dicks on the German character and Nazi military organization may be cited as an example. But most jobs in psychological warfare seem to call for maximum fertility in ideas and "hunches". They provide the minimum of opportunities for critical consideration or experiment. And these are not conditions in which the majority of research psychologists tend to be at their best.

If military psychologists cannot be expected to eliminate war and if they are not usually even very good exponents of the art of psychological warfare, what *do* they do? The short answer is that they are occupational psychologists, who have helped to show the way to considerable economies in manpower and in human effort in the Services. It is for this reason that they have now become permanent members of the British defence departments.

But the short answer requires to be extended and illuminated by reference to typical activities; and for this purpose a tabular summary has been made and is reproduced below. The summary refers to the activities of the Admiralty's group of psychologists only; but these may be taken as reasonably representative, and in any case examples from the work of other groups are given in the following paragraphs.

The summary (Table I, overleaf) shows that psychologists have been concerned in the main with the individual consideration of the fighting man and his job. Their activities can be grouped under two main headings: "Fitting the man to the job" and "Fitting the job to the man". In "Fitting the man to the job", they have helped to bring about improvements in:

(i) methods of personnel selection and allocation,

(ii) methods of training and examining and reporting, and

(iii) methods of documentation of Service men (and women).

In "fitting the job to the man", they have been concerned in helping in the improvement of:

Table I. Functions of the Admiralty Psychologists

The Admiralty Psychologists are industrial psychologists (*not* psychiatrists) and are therefore concerned chiefly with *man-power economy*. Their interest is in

- "fitting the man to the job" by helping in the
  - improvement of methods of personnel selection
    - e.g. for electrical and engineering branches
    - ——in recruiting centres
    - ——in entry establishments
    - ——in depots and specialist schools
    - ——on Admiralty Selection Boards
  - improvement of methods of training and examination
  - improvement of documentation
    - e.g. for naval recruiting
- "fitting the job to the man" by helping in the
  - improvement of methods of work
    - e.g. gun drills and methods of storing
  - improvement of design and layout of equipment
    - e.g. Action Information Organization
  - improvement of working and living conditions
    - e.g. lighting and interior decoration of ships

Notes: 1. In general, our function is *advisory*, and nobody need take our advice if they don't like it.

2. In most of our activities we work as members of a *team* in which the user branches are strongly represented.

3. In the personnel selection field, we have responsibility for the technical training and supervision of both *naval recruiters* and *personnel selection officers* (PSOs), and we provide a psychologist for all boards concerned with the selection of cadets.

(iv) methods of work,
(v) design and lay-out of equipment, and
(vi) working and living conditions.

In none of these improvements have psychologists been the only workers concerned. But in each they have had a part—and in several an important part—as researchers, advisers or technical executants.

Table I[1] cites an example of work under each of the headings (i)–(vi) above. To supplement these examples an additional one will now be given under each heading. They are not as up-to-date as might be desired, because, for security reasons, they have all been taken from published work. But they should at least be reasonably representative, since they come from the work of all three Services and from university work done for the Services.

PERSONNEL SELECTION

First of all, Service work in personnel selection may well be represented by the Army's very extensive and successful experience of selecting men for training as tradesmen. The advantages of using psychological methods applied by Personnel Selection Officers trained and guided by psychologists is brought out in Table II (taken from Vernon and Parry's "Personnel Selection in the British Forces").

It will be seen from Table II overleaf that even the adjusted failure rate—which takes into account the fact that different training categories had different rates of failure—is greatly reduced when men are nominated for training by Personnel Selection Officers. Self- nomination, nomination by commanding or technical officers and nomination by Ministry of Labour officials all achieve a rate nearly double that of the Personnel Selection Officers.

[1] Acknowledgments are made to Mr. Alec Rodger, who drew up this table when he was senior psychologist to the Admiralty.

TABLE II

FAILURE RATES OF ARMY TRADE TRAINEES SELECTED BY DIFFERENT METHODS

Method	*Number of men*	*Failure rate*	*Adjusted failure rate*
		per cent	*per cent*
Nomination by Commanding Officer or Technical Officer	3,566	17·0	19·2
Nomination at own request	3,176	16·6	19·6
Nomination by Ministry of Labour	911	27·1	19·4
Nomination by Personnel Selection Officer	2,201	8·7	11·1

TRAINING

In the second place, C. B. Frisby's researches in flying training will serve as an example of psychological work on training methods and problems. They were carried out when he was acting as Air Ministry Consultant at the Empire Central Flying School, and are described in the issue of *Occupational Psychology* for January, 1947. Frisby's central problem was the accurate assessment of flying skill at various stages; this was essential in order to evaluate not only the progress of individuals but the effect of changes in methods and equipment used in training. Simple as his problem was in essence, it called for an immense volume of systematic experimentation and analysis of results. And in the end it proved to be almost as intractable as it was

important. To put the matter in over-simplified form, the more objective and reliable assessments were made, the less relevant they seemed to become; while impressionistic methods adopted in an effort after realism and validity became less and less reliable as they were made more realistic.

DOCUMENTATION

The third example concerns documentation of Servicemen and is drawn from Royal Naval practice. Official documentation is necessarily devoted mainly to selected events within the Service life of the man concerned; and these events are for the most part recorded for their legal or financial significance. But Service psychologists are, equally necessarily, concerned to obtain a different kind of record, more comprehensive, extending back into the Serviceman's educational history, and "cumulative" in nature. A short biographical record form of this kind was constructed in the earliest days of the Admiralty psychologists and has been in use now for just over ten years. Its function is to summarize as many as possible of an individual's achievements and interests which are occupationally relevant. Of its validity there is space to quote but one instance. A highly experienced Personnel Selection Officer, a woman, using the form as sole guide was able to grade the suitability of some 500 men for a certain training course and obtain a validity coefficient of ·554; whereas the best that the standard test battery could do was indicated by a validity of ·393. Her correlation with the tests was ·312. From this it seems clear that the Personnel Selection Officer was able to use the form effectively as a means of gauging character traits and interests relevant to the job. Admittedly this was a very good Personnel Selection Officer; but it does seem doubtful whether any other form dealing with biographical record and interests, which can be adminstered in fifteen minutes, has shown itself capable of yielding such useful information.

METHODS OF WORK

With the fourth example we leave "fitting the man to the job" and go over to "fitting the job to the man". The example is of economies resulting from improved methods of work. Many of the successes obtained in this field have been due to the efforts of motion study scientists and engineers who have usually worked in close association with the Service psychologists. And the simple example cited is from the record of the Naval Motion Study Unit. The Unit made film studies in the usual way of the drills employed in loading a certain type of gun. They were able to demonstrate that, after certain rearrangements, the gun's crew could be reduced by two men while the loading time was unaffected. They were later able to repeat this result exactly with another important type of gun, so that their efficiency was not dependent upon any unusual feature of the first gun or the drills originally used with it. The contributions of psychologists working with motion study experts in the Services have, for the most part, been of two kinds. First, they have advised on design of experiments and criteria for evaluating proposed changes, whenever these have been needed. And secondly, they have advised on the needs and capacities of users of equipment. The aim of all their advice has been to ensure that all drills and equipment prepared for Service use show the maximum possible adaptation to the needs and capacities of all probable users in all probable operational circumstances.

DESIGN OF EQUIPMENT

Efforts to improve working methods more often that not suggest or necessitate improvements in design and lay-out of equipment. An example of this kind of work is that afforded by Wing Commander Ruffell Smith's investigation of human factors in the design of aircraft cockpits (described in *Occupational Psychology* for April, 1950). Ruffell Smith tells how

efficiency can be increased and accidents avoided by proper marshalling and grouping of controls; by arranging that controls should act, and react with "displays", in "normal" and "expected" ways; by choosing the best kinds and combinations of displays; and by making other arrangements to enable the pilot to "appreciate" with maximum rapidity and accuracy the relevant conditions both inside and outside his aircraft. The value of proposed changes in a cockpit can often be determined by quite decisive experiments. But it is interesting to note that Wing Commander Ruffell Smith has on occasion expressed a considerable faith in the qualitative tests of flight trials and the opinions of experienced pilots.

WORKING AND LIVING CONDITIONS

Finally, improvements in working and living conditions. Here some military psychological work done outside the Services by N. H. Mackworth and others under Sir F. Bartlett's direction, at Cambridge, may be cited. In his report recently published by H. M. Stationery Office, Mackworth has described some simple but decidedly cogent researches on the effects of various working conditions on the efficiency of Service "operators". He has suggested, among many interesting findings, that the efficiency of the average asdic or radar watchkeeper may normally be expected to fall by fifty per cent after the first quarter of a two-hour watch; and has shown how such a serious loss may be obviated by adopting a system of thirty minute alternations, watch-on with watch-off. In another section of the same report he shows how the efficiency of work often falls off quite sharply at effective temperatures exceeding about 87°F. A particularly interesting finding is that, in highly skilled tasks the real expert holds up better than the moderate or poor practitioner against adverse conditions. The explanation, which has considerable practical significance, is apparently that the expert

is able to work with little expenditure of effort, but not so the non-expert.

So much for examples of what has been done and is being done. In what directions are advances to be expected, what are the growing points? Five may be mentioned from among the numerous topics which seem likely to provide work for military psychologists for a long time to come.

POSSIBLE DEVELOPMENTS

First, manpower planning in terms of quality is now beginning to become possible in a tentative and cautious way. Given the composition of a Naval entry in terms of classification test scores, and given an approximately stable and known connection between these scores and advancement in rating, *approximate* estimates can be made of, say, the number of potential petty officers at the present standard of petty officer ability in any given category. In the same way it is now easier than formerly to compare the relative values of Boy, Youth and Adult entries to the Seaman Branch of the Royal Navy. At present, the authorities are rightly cautious and tend to demand supporting collateral evidence. But it can hardly be doubted that such estimates have come to stay and that, after their reliability has been improved, they will win general acceptance.

Second, manpower planning in terms of quality always shows that there is not enough of it. Therefore considerable attention is likely to be given to the "utilization of marginal groups"; the very able or unusually qualified; the dull; the physically sub-standard; the "high-grade funnies"; the unstable —all such groups can repay very considerable thought.

Third, within the Services (and, it may be suggested, between the Services and civilian industry) there are a good many organizational arrangements which will repay looking at in the light of what is now known about human abilities and interests. In one Naval category, for example, it has been found convenient

to combine two types of duty each of which calls for aptitudes which are psychologically distinct and may be mutually incompatible. In such a case, of course, the decision is with the administrative authorities as to where the balance of advantage and disadvantage lies. But "job analysis" investigations can often make important contributions to the evidence on which such decisions are based. And it is known that a very fair amount of psychological effort is going into job analyses both in this country and elsewhere.

Fourth, a considerable and very useful amount of effort by Service psychologists can be broadly called "educative". Making known the advantages of "over-learning" skills and of "knowledge of results" to training; and of the proper co-ordination of control adjustments with related display changes to equipment design, are two specific examples. Then there is the further *ad hoc* training of Personnel Selection Officers to give day-to-day advice on selection work which the psychologists are too few to keep in constant touch with themselves. Not least in importance is the provision of information about selection and the uses of applied psychology in warfare for the ordinary man in the tank or ship or aircraft. Although in the last war the British Forces did not, as the Americans did, supply a special Penguin book on military applications of psychology, much work was put into complying with requests for information. And it was very useful work, because "new-fangled" techniques cannot be applied with full effectiveness until "every-man" has got some grasp of what they are about and how they affect him.

Fifth, attitude and opinion surveys have, for obvious reasons, been infrequently employed in the Services. But they can on occasion be exceedingly useful in informing executive action. And there can hardly be doubt that, if their exponents use skill and discretion, such techniques will gradually gain an assured, if limited, place in Service work. *The American Soldier* studies have blazed a useful trail for such a development.

In conclusion it may be noted that each of these five developmental topics is very much of an "applied" kind. Service psychologists are required to be research technologists rather than pure scientists. But it does seem important to emphasize that the solution to each problem which has been solved so far has depended in the first instance upon some quite general development in psychological or statistical work. And, for that matter, the subsequent applications have seldom been routine affairs. They have quite often involved extensive field researches in their own right or at least further experimentation in the scientific sense. The Services therefore have a need for psychologists who are competent theoretical men and methodologists. Narrow topic-specialists, whether they be personnel selectors, training researches or design-of-equipment experts, will not suffice.

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IV

APPLICATIONS OF PSYCHOLOGY IN THE CIVIL DEPARTMENTS

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PSYCHOLOGISTS IN THE CIVIL SERVICE COMMISSION

Most of this paper will be concerned with psychologists in the Civil Service Commission. The Commission is responsible for the recruitment of all established Civil Servants, but not, as for instance in Canada, for what happens to successful candidates after they have entered the Service. Until 1950 there were two separate establishments employing psychologists, the Civil Service Selection Board (CISSB) at Stoke d'Abernon and the Research Unit (RU) in London. At the beginning of 1950, however, CISSB moved from its "country house" to a London house, and the two units are now housed in the same building.

CIVIL SERVICE SELECTION BOARD

The activities of CISSB have been fully described by N. A. B. Wilson, who served on the Board as psychologist in charge from 1945 to 1950. To the extent that they form a minority of the CISSB directing staff and are not represented on the Commissioners' "Final Selection Board" (FSB), psychologists play

[1] My thanks are due to the Civil Service Commissioners for permission to quote follow-up results, and to all members of the Research Unit who have done the work on which this paper is based. The opinions expressed, however, are entirely my own and are not necessarily shared by the Civil Service Commissioners or by any government department. E.A.

only a subordinate part in the procedure. They have, however, made a distinctive contribution in three ways:

First, they took a prominent part in the planning and setting-up of CISSB. Second, they brought to CISSB a certain expertise in the scientific study of personality, derived from training and practice both in the construction and evaluation of tests and in interviewing.

Third, and this in my opinion is their most important contribution, they insisted from the first on the vital necessity for a properly planned follow-up, in order to provide objective validation of what was in 1945 rightly regarded as a purely experimental procedure.

The results of the first main round of follow-up, given by Vernon in the April 1950 number of *Occupational Psychology*, were distinctly encouraging and justified the decision to continue the use of CISSB in "Normal", as opposed to the special "Reconstruction", competitions for a period of ten years. It must be remembered, however, that the results were based on short-term follow-up reports after one and two years' service, and only long-term follow-up reports after ten or twenty years' service will show whether CISSB has been as good in picking people who will shine in the higher posts as in picking satisfactory Assistant Principals. Moreover, whereas "Reconstruction" candidates were up to thirty-three years in age, "Normal" competition candidates are all less than twenty-five years old, inexperienced and therefore more difficult to assess. "Reconstruction" candidates were seen by an exceptionally able team of assessors who changed little over a period of four years: CISSB staff now reduced in numbers and change at frequent intervals, so that continuity of method and standard has become more difficult to maintain. "Reconstruction" follow-up findings are thus inapplicable to CISSB operating under substantially altered circumstances, and an entirely fresh follow-up of "Normal" competition entrants is contemplated in about 1957.

RESEARCH UNIT, FOLLOW-UP

The main activities of the Research Unit or RU have been job analysis, test construction, and follow-up.

It is interesting to compare with Vernon's CISSB follow-up the results for the first hundred "Limited Competition" entrants to the Administrative Class on whom follow-up reports were received after two years' service. These LC candidates, all serving Civil Servants, did not pass through CISSB, but took a written examination carrying 600 marks and were given an independent Board interview carrying 400 marks, preceded by a personal Pre-Board interview. The follow-up was interesting methodologically because as many as twelve different criteria were extracted from the mass of data on the rather elaborate report form. Of these twelve, the two found most useful were:

D, the Overall Grading for present performance, and

F, the estimate of Future Promise.

As this work, which was done mainly by Mr. A. H. J. Baines, then RU statistician, has not been published, I should like to indicate three main findings:

1. The validity of the whole procedure was satisfactorily high, with D as criterion ·64, with F as criterion ·53.

2. The written examination made a very useful contribution, especially in predicting cognitive ability, as assessed by the sum of nine relevant items on the report form. But selection would have been more efficient if equal weight had been attached to the interview, which made a much better prediction of quality of personal relations, as assessed by the sum of four relevant items.

3. No part of the selection procedure made an accurate prediction of strength of will, as assessed by the four remaining items on the report form.

For the higher grades in the Civil Service, the results of short-term follow-up must be regarded as highly tentative. At the

lower levels, however, where the work is more routine, a reasonably accurate assessment of a new entrant's efficiency can be made after a relatively short time. In Clerical Class competitions there is no interview, and the most interesting comparison is between the validity of "old-style" English, Arithmetic and General papers etc. and "new-style" tests. In 1948 a sample of over 3,000 serving Clerical Officers, whose scores in the existing old-style entrance papers were available, were given a battery of fourteen short experimental new-style tests, and confidential reports were obtained on their efficiency. Length of clerical experience was included as an additional variable and "partialled out" of the criterion. The three most important findings were:

1. The "contest" between the old-style papers and new-style tests resulted in a draw, the validity of either battery for the larger group of 2,419 "Reconstruction" entrants being about ·34.

2. The efficiency of the present method of selection by old-style "academic" papers would not be increased significantly by adding a general intelligence test.

3. The efficiency of the present method of selection would, however, be increased significantly (though not very much) by adding two new-style tests, one verbal and one numerical. Moreover a comparison of scores in these two tests could be used by the employing department as a pointer to relative suitability for mainly verbal and mainly numerical types of clerical duty.

Very similar results were obtained from an experiment carried out in 1947 on 279 Clerical Assistants.

RESEARCH UNIT, SELECTION PROCEDURES

I should like to stress the advantages of close collaboration between people experienced in old-style and new-style methods of examination. Some new-style techniques, e.g. that of item analysis, could be adapted to the improvement of old-style

papers. Psychologists have something to offer even in the intractable problem of increasing the reliability of an essay paper, on the lines suggested, for instance, by P. E. Vernon or by Verner M. Sims. On the other hand, with regard to the content of their test items, the new-style test constructors in RU have profited greatly from the help of people experienced in setting old-style examination papers.

In the Civil Service Commission at the present time, for school- and university-leavers at the clerical and higher levels, old-style examination papers are considered to be more suitable. Even where there is a shortage of candidates, other factors such as the greater acceptability to most candidates of the old-style examination procedure are of decisive importance. At the sub-clerical levels, on the other hand, the shortage of candidates and consequent need to speed up the examination process as much as possible have tilted the scales in favour of new-style tests. Clerical Assistants are recruited entirely and trainee-typists almost entirely by means of new-style tests.

From numerous experiments and follow-ups, we have drawn eight general conclusions as to the relative merits of different types of new-style tests in selecting Civil Servants.

1. General intelligence tests, though they may have moderately high validities and be worth using when time is short, are less useful than tests with a verbal or educational content or specifically related to the work of the grade.

2. Non-verbal tests of intelligence are almost useless, though non-verbal tests of specific aptitude may on occasion be useful.

3. At the administrative level, the best cognitive tests are the General Information (Current Affairs) test and tests involving verbal facility and the capacity to distinguish between fine shades of meaning. The last type of test, however, requires skilled scorers and is very tedious to mark.

4. For Clerical Officers, Clerical Assistants and, to a lesser extent, Typists, arithmetic and other numerical tests have come out very well.

5. New-style tests of knowledge of English and verbal facility have come out particularly well for Typists.

6. Tests of comprehension of prose passages, possess high "face-validity" for Civil Servants and have therefore been tried out both at CISSB and at the clerical and sub-clerical levels. The results have been generally disappointing.

7. The "Practical Situations" tests have given consistently good results. In each item of this test a familiar situation is given, e.g. Mrs. Brown hurrying out of a grocer's shop, and from five alternative explanations the subject has to choose the most plausible one, e.g. that she has forgotten her ration books, rather than that she has suddenly lost her temper with the grocer. This test is popular with candidates, perhaps because they are pleased to find that Civil Servants are required to display common sense, and it is easy to score, but it is hard work thinking up a sufficient number of fresh situations to keep pace with the demand for new tests.

8. For the sub-clerical and manipulative grades, tests of accuracy, e.g. checking surnames and Christian names against signature, have been shown to be valuable.

One of the most striking recent improvements in selection procedure has been the introduction of a new scheme for the recruitment of established Postmen. Hitherto men had been taken on as temporary Postmen on the strength of a short interview, but in order to become established had to pass a Civil Service Commission examination in English and arithmetic after they had served for six to twelve months. Under the new scheme introduced in March 1951 candidates take three short aptitude tests, two of accuracy in carrying out instructions and one of observation. These tests are quickly marked on the spot, and candidates have to reach a certain pass mark before they qualify for interview. The interviewing Board of two Post Office representatives take account both of the aptitude test scores and of their own interview impressions in deciding whether to pass or fail a candidate. If they pass the candidate, he

becomes established within a few weeks, subject only to passing a medical examination and his character references etc. proving satisfactory. A candidate can be tested, interviewed and, if successful, measured for his uniform, all within an hour and a half.

RESEARCH UNIT, OTHER FIELDS OF INTEREST

Hitherto the psychologists in the Civil Service Commission have concentrated almost exclusively on problems of selection. It is, however, arguable that more effort should be spent on studying how to get the best work out of the staff when selected, that is, to the problem of incentives. This problem is of even greater importance in the Civil Service than in private industry, since the best known incentives of the "stick and the carrot" have less obvious application, and research in the Civil Service must be restricted to the intangible, though equally important, non-monetary incentives. Certain relevant experiments have been carried out in the United States by members of the Survey Research Centre of Michigan University under D. Katz and in this country by R. Marriott. They have shown variations of the order of 2: 1 between the productivity of similar groups of workers employed on identical work. As the groups did not differ greatly in respect of the innate abilities of their members, this difference could be attributed only to the different psychological structure of the groups. If variations in productivity of this order exist in the Civil Service and if means could be found of raising the productivity of the less efficient groups to the level of the more efficient groups, the practical dividends would be very great.

A good deal of attention has already been paid to one particular incentive, namely the design of staff report forms. Establishment Officers and reporting officers in many departments have emphasized the vital importance in building up good morale, of having a staff reporting system which not only is as comprehensive, accurate and fair as possible, but also is recognized as

such by the staff. In my own department, for instance, at the end of 1950 there were conferences on staff reporting to which all supervising officers were invited, and at which talks were followed by questions and animated discussion. These conferences produced a marked shift in opinion. For example, whereas before the conferences only sixteen per cent of supervisors thought that standards of reporting were as uniform as practicable between different branches in the department, three months after the conferences sixty-six per cent were satisfied that this was so. Apart from enquiries into reporting, however, comparatively little has been done. The most profitable lines of future enquiry might be:

1. study of the "working group", its optimum size for various kinds of work, its internal organization, relations between supervisor and staff, frequency of transfer of staff etc., and

2. study of "internal incentives", that is methods by which an individual can be helped to make a regular self-assessment of the quantity and quality of his work (cf. Bartlett). A simple example consists of the systematic cross-checks that can be self-administered while doing lengthy arithmetical calculations: regular confirmation of the accuracy of his work maintains the self-confidence of a computer and thus enables him to work faster and with less strain.

PSYCHOLOGISTS IN OTHER CIVIL DEPARTMENTS

I regret that space permits only brief references to the work of psychologists in other Civil Departments. The work of psychologists associated with the Ministry of Labour has been described above by Rodger. The other main user is the Prison Commission. In accordance with recent legislation it is intended that psychologists should participate both in the initial scrutiny of persons receiving a prison or Borstal sentence and in any special treatment that seems called for. There is a small, but rapidly expanding, staff of clinical psychologists, including two Principal

Psychologists, aided by psychological testers, under the control of the Director of Medical Services. The Principal Psychologists are at present employed at the two Borstal Allocation Centres, where boys stay until the Board has decided to which of the four "closed" Borstals or ten "open" Borstals they are to be sent. Apart from the obvious factors of whether the Borstal is open or closed, big or small, there are a lot of other variables which are taken into account by the Allocation Board, for example, the kind of training facilities available, whether the discipline is hard or relatively soft, whether there is a strong religious atmosphere or not, whether boys of above average, average or below average intelligence are normally sent there, and the special abilities of the Borstal staff. Every endeavour is made to "marry" the boys with the most appropriate Borstals, and a collection of the reports prepared by members of the Board goes to the Borstal Institution with the boy, so that the staff there can have the benefit of the Board's diagnosis and advice. The Principal Psychologists spend most of their time interviewing boys waiting for allocation, and their advice carries a good deal of weight with the Board. A follow-up has just been started, with a view to checking the accuracy of the diagnoses. This takes the form of a report back to the Allocation Centre from the Training Borstal when a lad has been discharged. Other psychologists are employed on the allocation of prisoners to corrective training, the collection of information for an "Investigation Board" of Borstal recalls, i.e. boys who have gone astray again after their first release, similar duties in connexion with the relatively small number of female prisoners, and the selection of new Prison Officers. A very few psychologists work also in other departments, e.g. in "Social Survey".

OUTLOOK FOR THE FUTURE

It must be admitted at once that, for psychologists as for everybody else, the high hopes of 1945, that there would be

steadily increasing scope for psychologists in the task of peaceful reconstruction, have been only partially fulfilled. Psychologists must expect cuts in research on matters not directly connected with defence, and in the Civil Service Commission they will be asked to concentrate on making selection simpler, quicker and cheaper, rather than more valid. Quite rightly in my opinion, attention will be concentrated more on the lowlier grades such as Typists and Postmen where the shortage of suitable candidates is acute and where a substantial speeding up of the selection procedure would be most valuable in itself and might also have the effect of attracting more people to compete.

There are three points which psychologists employed in the Civil Departments might well take to heart.

First, be practical. Before planning any research project, it is necessary to be clear as to the practical benefits likely to be derived from the particular piece of research, to weigh these benefits against the time, trouble and money likely to be expended on the project, and to be satisfied that there is a reasonable chance of convincing potential critics that the project is worth while.

Second, be intelligible. The main report on any investigation must contain sufficient evidence in support of its conclusions and recommendations to satisfy the technical reader, and this necessarily implies the use of many technical terms. The non-technical reader, however, cannot be assumed to have more than a nodding acquaintance either with psychology or with statistics, and for his benefit it is necessary that the report should be preceded by a non-technical summary which indicates briefly what has been done and why it has been done, and gives the main findings and the recommendations for action in accordance with those findings.

Third, in a research unit it is necessary to strike a satisfactory balance between a high professional standard and practical effectiveness. If, for example, one of the psychologists is severely practical and anxious to get on with a job, he needs

to be kept in check by another psychologist who is relatively academic and purist in approach, and vice versa. A professional statistician is an indispensable member of the team, but he is bound to demand the largest possible samples in an experiment in order to increase the chance of obtaining statistically significant results. Equally indispensable, therefore, is a staff officer able to estimate how much clerical work and computing can be done by the available staff in the available time, and thus to keep plans within the limits of what is attainable in practice.

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V

EDUCATIONAL SELECTION AND ALLOCATION

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SELECTION AND ALLOCATION OF CHILDREN FOR SECONDARY EDUCATION

In this, the main section of the paper, a brief account will first be given of the methods used by most local education authorities; second, an attempt will be made to answer the question "How good are these methods when judged by following-up the children later?"; third, a summary account will be given of recent research and developments which bear on the question, "How can these methods be improved?"; and fourth, suggestions will be made for further research and development.

SELECTION METHODS USED BY MOST LOCAL EDUCATION AUTHORITIES

One point needs emphasis at the outset: the procedure used at the 11+ and 12+ transfer stage cannot be called "allocation to secondary education" but is, in fact, "selection for Grammar Schools" (in Scotland called Senior Secondary Schools). Not until there is something approaching parity of esteem—in the parents' eyes—between Grammar, Technical and Modern Schools (or courses) will "selection" become "allocation".

It is possible, from the excellent report of the National Union of Teachers' Consultative Committee to construct a picture of the normal Grammar School selection procedure used by

local authorities in England and Wales. Though there are considerable variations between different areas, the general lines are as follows: all children in the eleven year old group are reported on by their Primary School headteachers—the reports including estimates of intelligence, attainment, special abilities, interests and personal qualities. The reports are received by the Education Office staff who (under an appropriate sub-committee of the local authority and assisted by some teacher-representatives) are responsible for the executive work connected with selection. (The Education Office staff may or may not be specially qualified or interested in this job.) Standardized tests (e.g. Moray House tests) of intelligence, English and arithmetic are obtained from the publishers and given to the children by their teachers. The tests are marked by panels of specially selected teachers, the raw scores so obtained are converted to standard scores which include an age allowance and the three separate standard scores (equally weighted) are added together. This total for each child determines his position in the order-of-merit for filling the available Grammar School places. The report from the school and the results of an interview—usually by a panel—are taken into account for border-zone children.

In Scotland the normal selection procedure, taken at 12+ instead of 11+, is on very much the same lines as in England and Wales but with these differences: 1. teachers' estimates of their children's attainment are scaled (by the method described by McClelland) and added to the three test standard scores; 2. alternative types of courses (e.g. academic, commercial, technical, domestic) are available *within* the Senior or Junior Secondary course for which the child has been selected.

FOLLOW-UP STUDIES OF SELECTION PROCEDURES

"How good are these selection methods?"

Most follow-up investigations of Grammar School pupils

take as the criterion the marks obtained in all or some of the subjects in the Grammar School curriculum. Research workers able to obtain later results have taken School Certificate results as the criterion. No one is happy about the adequacy of these criteria: most people would question the assumption which equates a child's "ability to profit from a Grammar School course" with his position on a mark list. Watts and Slater have set out in the first part of their report the many objections to taking School Certificate as the criterion and suggest, instead, 13+ (i.e. after two years in the Secondary School) as a better criterion of "ability to profit". I cannot see that this will answer their own objections as, presumably, they would wish to quantify the two year follow-up results and hence would need to use marks, scores, gradings and the like as criteria.

However, for what they are worth, these typical follow-up results have been obtained.[1]

1. Emmett reports a two year and a three year follow-up of West Riding children who had been selected on a Moray House Intelligence Test and an unstandardized English and arithmetic examination. The criterion was the Grammar School headteachers' order-of-merit. The two year follow-up yielded a multiple correlation of ·743, and the third year a multiple correlation of ·764. Of the three tests the Moray House Intelligence Test gave the best single prediction of the Grammar School order-of-merit.

2. McClelland followed up, after three years, children selected in 1936 for Dundee secondary schools. The best prediction of overall success in a Senior Secondary (i.e. Grammar) School course (multiple correlation of ·8) would have been obtained by using these in the initial selection: English and arithmetic examination, teachers' scaled marks, standardized attainment tests and intelligence tests. As single predictors, they come in the order stated.

[1] The correlations quoted are corrected for selection, unless otherwise stated. Within actually selected groups, the accuracy of prediction is considerably lower.

3. Emmett (in an unpublished paper) obtained "follow-back" data on two groups of Huddersfield children who took School Certificate in, respectively, 1946 and 1947. The follow back data for both groups comprised Moray House Intelligence, English and arithmetic tests taken, respectively, in 1941 and 1942. The criterion for each group was the pooled School Certificate results in English Language, English Literature, History, French and Maths. The multiple correlations obtained were ·849 for one group and ·830 for the other.

4. Rutter similarly correlated School Certificate results in 1948 with initial selection test results five years before (Moray House Intelligence Test) and obtained a multiple correlation of ·71. The criterion here was the School Certificate results in English Language, English Literature, Maths, and Geography. (This multiple correlation of ·71 is uncorrected for selection and must therefore be somewhere in the region of ·9 if corrected.)

5. Selection procedure which I carried out in Cornwall in 1947, 1948 and 1949 yielded first year follow-up multiple correlations of respectively, ·842, ·895 and ·915. In each case the criterion used was the total marks obtained in *all* subjects at the end of the first year in the Grammar School. In 1947 the test order of prediction was: Mechanical Arithmetic, Problem Arithmetic, Verbal Intelligence, Vocabulary, and Non-verbal Intelligence. In 1948 the test order of prediction was: Verbal Intelligence, Vocabulary and Mechanical Arithmetic. In 1949 the test order of prediction was: Problem Arithmetic, Vocabulary, Essay and Non-verbal Intelligence.

From these and similar investigations it would be unsafe to generalize but a few tentative conclusions can be drawn. They are:

1. That verbal intelligence tests are superior to non-verbal intelligence tests in predicting Grammar School courses—anyway over the short term.

2. That verbal intelligence tests alone can predict to the extent of at least ·7 correlation over the short term *and* as far as School Certificate.

3. Attainment tests (particularly arithmetic) stand high in the order of prediction, and vocabulary tests (i.e. tests of more or less pure "v") are worth including in selection batteries.

4. Tests of the old-style examination paper type, *if carefully set and marked*, can be good selection tools. (See McClelland's results).

To generalize even further: it can be asserted with fair confidence that selection for Grammar Schools can be competently done nowadays by the use of three, four or five standardized tests.

To illustrate this I will show what a correlation of ·842 (the lowest of the three Cornish results just quoted) looks like in terms of children's performance—in the tests and in the Grammar School curriculum.

INITIAL TEST SCORES	*Class position at end of first year in Grammar School*			*TOTALS*
	Top $\frac{1}{3}$	*Middle* $\frac{1}{3}$	*Bottom* $\frac{1}{3}$	
Top $\frac{1}{3}$	130	66	15	211
Middle $\frac{1}{3}$	53	87	70	210
Bottom $\frac{1}{3}$	28	57	126	211
TOTALS	211	210	211	632

I will return to this later under the heading of suggestions for further research and development.

RECENT RESEARCH ON THE IMPROVEMENT OF SELECTION METHODS

Primary School Teachers' estimates, ratings and reports. An obvious suggestion and one which appeals to all of us is to use

the Primary teacher's first-hand knowledge of the child. Recent research results (notably McClelland, Taylor, R. J. M., Taylor, E. G. and McFarland agree in showing that teachers' estimates of attainment, *when scaled on I.Q.*, are among the best predictors of Secondary School success. But evidence of the predictive value of teachers' assessments of character and temperament is not so encouraging. McFarland did find that ratings for character added significantly to the prediction of Modern Secondary School success but other investigators (Nath, Clark, Taylor, E.G.) found them to be of insignificant additional forecasting value. McClelland indeed, discovered that, overall, the number of misfits (i.e. admit-fails and reject-successes) would have been increased if the teachers' forecasts, their ratings for industry and their remarks on personal qualities, had been used.

The Essay. A written English Essay as an additional test in Grammar School selection procedure has been experimented with in Devon, Cornwall and Edinburgh during the last few years. Quite apart from the search for greater predictive efficiency, the reasons for including an Essay are: (1) to try to ensure a minimum standard of written literacy for all Grammar School entrants; (2) to try to combat the alleged tendency of Primary Schools to give scant practice to the exercise of written English because (it is again alleged) of the influence of the new-style tests used in selection procedure. Evidence from all three researches is encouraging about the possibility of markers—whether experienced markers or Primary School headteachers—being able to judge children's essays *consistently*. Both Wiseman and Finlayson report correlations around ·94 for a pooled result of a team of four markers marking essays on one occasion and re-marking them later. McMahon found that three psychologists had an average agreement intercorrelation of ·94 when ranking in order a sample of a hundred essays; also that ten headteachers had an average difference of only one-fifth of a grade (a grade being A, B, C, D, or E) when grading a random sample of essays. Six Primary School headteachers marking their own children's

essays had correlations ranging from ·89 to ·99 between first and second markings. Finlayson, however, found that consistency correlations were considerably lower when the re-marking was done on the children's *second* essays. The pool of six markers marking the children's first essays correlated with the same pool marking the children's second essays to the extent of ·863. This drops still further (to ·786) when a randomly selected pool of markers did the re-marking. Finlayson estimates that this figure could be increased to ·88 if, in fact, as part of selection procedure the children wrote two essays and there were two teams of markers—each team marking one set of essays and their marks being pooled.

Finlayson's inter-correlations of four variables (Intelligence Quotient, English Quotient, scaled teachers' estimates in English, and pooled essay work) seem to indicate that the Essay is testing something different from what the other variables are assessing. What that something is follow-up can, perhaps, show. The only recent validatory studies I know have been carried out in Cornwall. In the 1948 selection procedure followed up in 1949, the Essay had a correlation with overall success in Grammar School of only ·51. The same Essay gradings yielded a correlation of ·66 after two years in the Grammar School. The Essay gradings in the 1949 selection procedure correlated ·82 with Grammar School success after one year. A few comments need to be added to these bare figures: (1) These validity coefficients—although promising—are below those obtained for standardized tests in the selection battery; (2) I would have expected the essay correlation to *fall* rather than rise in the second year follow-up; (3) The highest first year follow-up coefficient (·82) was obtained when Primary School head-teachers graded their own children's Essays by using facsimile templates of A, B, C, D, and E Essays.

Distinguishing Grammar and Technical Types. Another line of enquiry has been into the possibility of distinguishing able children who at eleven or thereabouts are more fitted for technical

rather than Grammar School courses. Lambert found that giving a combined information and interest test to 11+ children in Northumberland enabled her to calculate for each child a Practical *versus* Academic Index. This Index—together with Alexander's Performance Scale and Peel's Non-verbal Intelligence Test—contributed to the prediction of differential success in these two groups of Grammar School subjects: "The Academic Group" composed of English, French, Maths, and Science; "The Technical Group" composed of Maths, Science, Art, and Woodwork. Watts and Slater, applying different statistical procedures to Lambert's follow-up sample (sixty children), found that the Practical and Academic Index, Alexander's Performance Scale (when re-standardized), Peel's Non-verbal and Practical Tests all gave some help—over and above the usual selection tests—in the prediction of performance in specific Grammar School subjects. Earle in an unpublished paper reports extensive follow-up studies of children who had been given his Duplex Ability Tests. These four tests are designed to reveal differences between children both in level *and* in bias—the biases being linguistic on the one hand and technical, mathematical, scientific, practical on the other. By testing and by follow-up over three and a half years, Earle demonstrates (a) approximately ninety per cent agreement in the classifications one would have made from year to year on the basis of tests and (b) considerable—but, of course, less spectacular—agreement between such classifications and school records. It is important to note that the children were allocated to their courses without anyone knowing their Duplex Text results. To quote from Earle's paper: " . . . there is in all cases a definite tendency for the individuals who are graded by the tests in the Linguistic Group to obtain higher marks in English and in Languages than in Mathematics and Science subjects, whereas those . . . graded (by the tests) in the Technical Group display greater interest and efficiency in Mathematics and Science and (when studied) Technical subjects."

RECENT DEVELOPMENTS

The Quota Scheme. Several new developments have in common the desire to lower the emotional temperature of the transfer procedure, to make it more of an ordinary event in the lives of the children, to remove as much as possible from the external examiner and to put more on the Primary School teachers. One development is the "Quota Scheme" of allocating children to Grammar Schools. Its inventor was Professor C. W. Valentine, it has been described in practice by V. J. Moore, and been recommended to local authorities by the N.U.T. Consultative Committee. According to this scheme, the results of standardized intelligence tests decide the *number* of Grammar School places to be allotted to each Primary School; the actual *individuals* in each Primary School are then selected in accordance with an order-of-merit based on the intelligence tests plus school order-of-merit plus school internal examination. The plan, it will be noted, does not eliminate competition—it transfers it from "between schools" to "within schools". However, if (as is claimed for it) the system makes for a happier life for the Primary School children and their teachers, it has much to commend it. Of several possible disadvanatges of the scheme which occur to me there is time to mention only one—but one which would make it unworkable in rural counties with small primary schools. As I see it, a Primary School with no child above the intelligence test cut-off score will get no allocation; therefore no child in that school can be considered for Grammar School—even if they are "below the line" by only one point of I.Q. and well worthy of consideration in other respects. But—and this is the crux of it—similar children in Primary Schools in which places have been earned have a chance of being considered.

Preliminary and Practice Testing. Another development, for which there can be nothing but praise, also aims at mitigating the worst effects of the fateful D-day in the life of the child.

An increasing number of local authorities are now giving two or more intelligence tests in an attempt to get a more accurate assessment of the child's intelligence; some give them at intervals of a year, others at intervals of six months or three weeks. Also, practice tests and preliminary "knock-ups" are now being given, to make the children feel happier about it all and also in the hope of defeating the worst effects of coaching.

Second Chances. Some authorities are considering for transfer from Modern to Grammar School at 12+, children who—on any submitted evidence—can be called late developers or selection mistakes. From my own experience I would say that this is an essential part of selection procedure, whether or not a 13+ scheme is in operation.

Appointments of psychologists for selection and guidance. One of the most promising developments has been the appointment of psychologists for full- or part-time duties in selection and guidance. Three local authorities who have made such appointments are Northumberland, Devon and Cornwall. Northumberland has in consequence experimented with border-zone procedures and—as reported earlier in this paper—has carried out valuable research into the problem of technical *versus* Grammar School bias in children. Devon has also made elaborate experiments in border-zone interviewing procedures and has been the pioneer Local Authority in experiments with Essays. Cornwall has taken preparatory steps towards converting selection for Grammar Schools into educational guidance. These first steps have been: (1) teachers give standardized tests—held as school stocks—at times convenient to them and without most of the "official" fuss usually connected with selection testing; (2) these tests include non-verbal intelligence tests, vocabulary tests, mechanical arithmetic tests and Essay—all of which, while having high correlations with the Grammar School criterion tend to have lower inter-correlations; hence they are particularly useful for guidance and remedial work; (3) teachers mark these tests themselves (including the Essay),

converting the scores (where necessary) to percentile scores by using conversion tables; (4) teachers then enter these scores and grades on the Cumulative Record Card and on an *ad hoc* report form identical with the "Fourth Year Junior" page of the Cumulative Record Card; they then complete this very full report on the child. It is hoped that the cumulative record card, carrying the results of standardized tests over the years, will eventually abolish the need for either an *ad hoc* report form or any official testing. Educational allocation at appropriate ages should then be largely a by-product of continuous educational guidance.

SUGGESTIONS FOR FURTHER RESEARCH AND DEVELOPMENT

1. *Research* is needed into the causes of failure on Secondary School courses, particularly of failures who, on the face of it, should have succeeded. Cf. the Table on p. 49). Clinical investigation will be needed but with good scientific "controls".

2. More follow-up work needs to be done on the value of selection techniques in predicting *parts* of the Secondary School curriculum, e.g. Languages, Mathematics, Sciences. To that end more varied tests and, I would suggest, some with relatively pure factorial content (e.g. "v" tests) should be included in the experimental selection batteries.

3. Many more investigations are needed into predictive value of Primary teachers' knowledge of the children. Some researchers might use character sketches in words instead of ratings; others could make comparisons between the predictive values of assessments made by untrained teachers and those who have been on special courses of instruction in "what it is all about"; or between teachers in possession of objective test results on their children whom they assess, and those without such information.

Further development. Time does not allow me to enumerate all the developments I would like to see, but I will make one blanket recommendation which should cover most of them.

It is this: local authorities should press on with experimental methods which aim at bringing selection nearer to allocation and guidance and, to that end, they should consider employing psychologists—preferably full-time—whose duties would include the essential tasks of devising procedures, training teachers, interpreting test and other data, visiting schools, advising parents and following-up not only selection variables but actual, real, live children.

SELECTION FOR UNIVERSITIES

There is time only to summarize in the baldest fashion the results of recent research in this field. As far as I know, no student has been selected—or rejected—by British Universities on the results of special procedures devised by psychologists. All data relates to students already in residence. Himmelweit gave twenty tests to students at the London School of Economics and correlated the test results with degree marks. The four tests with the highest correlations, together gave a multiple correlation of ·53. Testing would, in fact, have given considerable aid in selecting good and rejecting poor students, in those particular years at that particular School. Petrie obtained slightly higher correlations when the same tests were given to medical students. Warburton reports lower correlations but this, as he says, may be due to the fact that his group (physics students) were a very highly selected sample. He found, however, like the others, that vocabulary tests and information tests were two of the most promising tests. Eysenck, in his excellent survey of American and British researches in student selection, agrees with the others quoted in concluding that prediction based on pre-entry examinations can, even now, be markedly improved upon—particularly at the border-zone of acceptance—by adding psychological testing. Now that applicants far outnumber the available places, and that large and increasing proportions of those accepted will be financed

by public funds, the suggestion has much to commend it. What is beyond argument is the desirability of widespread and continuous research into the validity of all selection techniques—examinations, school reports, interviews, tests, questionnaires. We shall never be able to forecast with certainty the academic future of an individual but we shall, at least, know just how uncertain an individual forecast is likely to be.

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VI

THE PSYCHOLOGY OF BASIC EDUCATIONAL TECHNIQUES

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THE past three decades have seen a rapid utilization of the findings of general and educational psychology in the classroom. More and more, too, teachers, usually as part of the requirements of study for a higher degree, have turned to the controlled investigation of their own classroom problems. The field is, however, large and ill-defined, and it is not surprising to find few authoritative text-books which attempt to cover the whole field of psychology in school. The Year Books of Education (Institute of Education, London), and the publications of the Scottish Council for Research in Education deal expertly and frequently upon the level of experimental research, with topical problems, but apart from these and the sporadic publications of individuals in particular fields we have little to compare in scope and intention with the numerous text-books of educational psychology and summaries of current research which are a feature of American education.

It is perhaps for this reason that much of the British work of the last two or three decades appears to fall into more or less distinct categories and that synthesis into a coherent body of doctrine is very much a matter of personal predilection and outlook. Such a state of affairs is, in the opinion of the writer, healthy, for if there is one inference to be drawn from current research it is that

many of the conclusions so confidently stated in text-books need extensive modification and that the optimism of those who believed that the study of child psychology could be reduced to a simple scientific procedure is largely unjustified. Correlational and case-study enquiries alike throw us back to the central psychological fact of the uniqueness of the individual pupil and the unreliability of causal generalizations which take no account of the dynamic interrelation of every aspect, social, emotional, spatial and temporal of a child's life. How this is so may be illustrated from a study of the findings of factor analysis as applied to educational success in the primary school.

FACTORIAL RESEARCH

The general upshot of this work is to suggest that a factor of "general educational capacity" (g + v:ed) is the main *cognitive* conditioner of success or failure. Two much smaller group factors concerned largely with the mechanical aspects of reading and of number do emerge and lend some support to the existence among children of more or less specific types of ability and disability. Burt,[1] to whose pioneer enquiries much of this work is due, is careful to point out that these "factors" represent, not an analysis of the minds of children but the relations between the children and their educational environment.

Similar conclusions emerge from factorial studies of reading and arithmetic. Richardson[2] identifies the general g + v:ed factor in reading and a small group factor appearing in tests concerned with the mechanics of reading. Sutherland[3] finds g, a verbal factor and a number factor as being of almost equal importance in the solution of mental arithmetic problems, a

[1] Burt, C., "The Relations of Educational Abilities". *British Journal of Educational Psychology*, ix, 1939, 45–71.

[2] Richardson, J., *A Factor Analysis of Reading Ability in Ten-year-old Primary School Children*. M.A. Thesis, University of Birmingham, 1949.

[3] Sutherland, J., "An Investigation into Some Aspects of Problem Solving in Arithmetic" *British Journal of Educational Psychology*, 1941, xi, 215–22; 1942, xii, 35–46.

finding largely confirmed by the work of Appleby[1], Cooke,[2] and Watt.[3]

This stress upon the general factor, as Vernon points out, lends little support to the intuitive analyses of educational skills upon which many of our attainment and diagnostic tests are based. Such tests of course retain their clinical usefulness as helping us to build up a factual picture of the ways in which a child is failing. In themselves, however, they give no clue—as their constructors hoped they would—to causes for failure.

Light upon this latter point is thrown by the further finding that factorial analyses of school marks and educational measures yield a different structure from objective psychological tests. It seems that when the child's response to education is measured and analysed, the g + v:ed component is heavily modified in learning by a complex orectic factor. This is an interesting confirmation of the current trend of work with individual failing children where the emphasis has shifted, as we shall see later, from the search for cognitive and physiological causes, to the exploration of the emotional implications of the child's environment as he sees it.

INVESTIGATIONS OF READINESS

Much the same kind of conclusion is beginning to emerge from studies of children's readiness for learning, from the work on age placement of topics in the curriculum and from the rather limited experiments with teaching method.

Reading readiness in particular has been the subject of a good deal of American work, and readiness tests have been published by Lee and Clark, van Wagenen, Hildreth, Stone and Grover,

[1] Appleby, E., *Factorial Analysis of the Development of Arithmetical and Other Abilities in Junior School Children*. Ph.D. Thesis, University of London, 1939.
[2] Cooke, K. S., *An Investigation of Some of the Factors Involved in Arithmetical Ability in School Children*. Ph.D. Thesis, University of London, 1933.
[3] Watt, G. C., "Correlations between Tests in Mechanical Operations and Reasoning Arithmetic". *Studies in Arithmetic*, i., University of London Press, 1939.

Monroe, and Gates.[1] Such tests have been found to be statistically reliable and valid but, and this is significant, not more effective predictors than intelligence tests and the judgements of trained teachers. English practice is perhaps best indicated by the criteria given by Schonell[2] who suggests that judgement should be based upon an assessment of a child's experience, information, emotional attitudes, general ability, and his level of maturity in the auditory and visual discrimination of word patterns.

There seems general agreement that a minimum mental maturity is essential for success and considerable attention has been paid to the determination of the appropriate age for the introduction of reading. The work of Morphett and Washburne is well-known in this field[3] and is the principal authority for the repeated statement that a mental age of six to six and a half is a minimum for probable success. Other enquiries have tended to confirm that, although an earlier start may be made with some immediate advantage, postponement is likely to lead to more solid gains later.

Analyses of errors made by beginners and by poor readers, especially errors in directional attack on words and in word discrimination suggested the existence of perceptual skills not directly connected with general intelligence. These, it was thought, matured more or less independently of teaching or practice. This view is strongly criticized by Inglis, and an enquiry by Taylor comparing the reading readiness levels of Scottish and American children certainly supports the suggestion that many of the factors hitherto considered to be maturational are markedly affected by practice.

Much the same kind of conclusion emerges from the Scottish work on the age-placement of topics in arithmetic. In his

[1] For a list of commonly reported American tests see *Studies in Reading*, i, 77. University of London Press, 1948.

[2] *The Psychology and Teaching of Reading*. Edinburgh: Oliver and Boyd, 1945.

[3] Morphett, M. V. and Washburne, C., "When Children Begin to Read"? *Elementary School Journal*, 1931, xxxi. Smith, C. A. and Jensen, M., "Educational Psychological and Physiological Factors in Reading Readinesss". *Elementary School Journal*, 1935, xxxvi.

enquiry, admittedly somewhat slenderly based, Curr found that, in general, mentally older starters learn better. He points out, however, that this is due in the main to the higher I.Q. of such as compared with their chronological contemporaries. He also found that, as in reading, one of the best predictive criteria of later success is mastery of the necessary foundations. In general, too, it seems that children who start earlier gain and retain a superiority over those who start later.

It must, however, be remembered that both the investigations of Washburne's Committee of Seven,[1] and that of the S.C.R.E. were undertaken with children who had been in school for a number of years. Neither enquiry was concerned with what might be called the pre-arithmetic period or with general readiness to profit from more or less formal teaching. Some light on this is thrown by the work of Flavell[2] who, using a predictive diagnostic battery of tests with children of 7+, suggested that addition is best commenced at a mental age of 6·5 years, subtraction at 7·5, multiplication at 9·5, and division at 9·5, and that these are fully matured at mental ages of 12 (addition and subtraction) and 14 (multiplication and division). Flavell's work is suggestive only. Being based upon one junior school and upon one test, it is more than usually vulnerable to the criticisms that teacher, method, and other variables affect the results to an unknown extent, and that arithmetic, consisting as it does of many processes, disparate both mathematically and psychologically, does not lend itself to generalizations.

It is notable that readiness studies whether of reading or arithmetic concur in finding that general educational ability is of importance but that its expression in actual learning of the necessary foundations for subsequent work is the best single predictive criterion. Such a conclusion can lead to two lines of

[1] See Washburne's *Adjusting the School to the Child*; and, Washburne, C., "Mental Age and the Arithmetic Curriculum", *Journal of Educational Research*, 1931, xxiii, 210–31.

[2] Flavell, J. D., *Arithmetic in the Junior School*. M.A. Thesis, University of Birmingham, 1942.

further investigation. It can lead to studies of the relative difficulty to children of processes involved and of the effectiveness of instructional procedures in, and for themselves: and it can lead to the immensely more difficult field of the study of children's attitudes and emotional responses as conditioners of their readiness to learn.

RESEARCH ON SCHOOL SUBJECTS

Studies of the first kind abound, and in this country the work of the Scottish Council for Educational Research is outstanding. The basic number facts in arithmetic, difficulties of zero combinations, errors in fractions, and the vocabulary of arithmetic have all been made the subject of research. In the field of reading, valuable contributions have been made by Vernon's analysis of the vocabulary of infant readers and the spoken vocabulary of Scottish children at school entry. Mention, too, should be made of Macauley's study of the ineffectiveness of grammar teaching in primary schools, and of the summary by Bagley of work in written English.[1]

A natural correlate to such enquiries and the criticisms they provoke are teaching method studies. Controversy over the appropriate methods for the early teaching of reading still rages and the apparent psychological rationale of the four or five main methods has been discussed by Schonell. Little rigorously designed experimental work, however, exists to determine the controversy; and in this field, perhaps more than in any other, teacher-pupil relationships are of crucial importance.

That this is so seems to be a legitimate inference from the two experiments with dull and backward readers reported by Burt and Lewis.[2] Using a rigorous experimental design based upon

[1] Macauley, W. J., "The Difficulty of Grammar". *British Journal of Educational Psychology*, 1947, xvii, 153–62. Bagley, D., "A Critical Survey of Objective Estimates in the Teaching of English". *British Journal of Educational Psychology*, 1937, vii, 57–71, 138–55.

[2] Burt, C. and Lewis, B., "Teaching of Backward Readers". *British Journal of Educational Psychology*, 1946, xvi, 116–32.

the analysis of variance, these investigators establish a significant difference in favour of a "look and say" method as compared with any other. But they also point out that success is obtained with all the methods used and that with the children studied a *change* of method was in itself beneficial.

Aspects of the teaching of arithmetic lend themselves rather more readily to the experimental examination of method without the difficulty of allowing for teacher and pupil variation. Here Murray[2] has obtained apparently conclusive results as to the best method of teaching subtraction. Equally ingenious is the study by Holmes[2] which demonstrated the value of self-teaching methods in learning multiplication tables.

Experiments of the kind alluded to and concerned mainly with more or less isolable processes are apt to be regarded by the practical teacher, with some justice, as academic. To him the gestalt of teacher, method and pupil and the emotional aspects of the whole approach adopted seem of more importance than purely intellectualistic considerations of technique. Unfortunately, studies which take account of orectic as well as cognitive variables in any exact way are difficult to make. Such studies as there are, however, made both in this country, and in America, seem to point to similar conclusions.[3] Most of them are concerned with activity methods which are claimed not to result in an inferior level of formal attainments but which produce in the pupils greater enthusiasm, co-operativeness, and a wider functional use of the basic skills.

[1] Murray, J., "The Relative Merits of Methods of Teaching Subtraction". *Studies in Arithmetic*, ii. University of London Press, 1941.

[2] Holmes, E. R., "Learning Multiplication". *Educational Review*, 1951, iii, 139–41.

[3] See D. E. M. Gardner's books, also: Curr, W. and Evans, E., "Intelligence and Attainment in an Activity School", *Educational Review*, 1950, ii, 215–21; Brearley, M., *An Enquiry into the Reading Tastes and Habits of 800 Children between 7 and 11 Years of Age*. M. A. Thesis, University of Birmingham, 1949; Gates, A. I. and Pritchard, M. C., *Teaching Reading to Slow Learning Pupils*. New York: Teachers College, Columbia, 1942; Hannicutt, C. W., "Reading of Children in Activity and Regular Schools in New York". *Elementary School Journal*, 1943, xliii.

BACKWARDNESS AND RETARDATION

Studies of errors and of teaching methods reflect our uneasiness over educational failure and during the past decades there have been many surveys of the incidence of backwardness and retardation in English schools and a number of case studies of groups of failing children. In general such surveys have confirmed the close dependence of educational progress in the junior school on general educational capacity—the majority of backward children prove to be dull and the primary cause of their low achievement is a lower than average intelligence. At the same time from all these surveys other facts emerge. Among the dull and therefore backward there is a number of children who are not achieving levels commensurate with their admittedly limited capacity. There are also children backward in school or having a specific educational weakness who are of normal or markedly superior ability.

Much of the work aimed at elucidating the causes of retardation among such pupils—especially American work—has been concerned with specific cognitive defects, defects of the special senses and more or less neurological correlates. And in general it has been inconclusive in discovering direct causal relationships. Visual and auditory defects, left dominance, cross laterality, speech defects, weaknesses in auditory and visual perception and the like can be shown to occur with greater frequency in backward and retarded groups than among normally functioning children; and, as such, can be held to be associated with failure. On the other hand many children with these and similar defects and difficulties do succeed and do well in school. In an individual case it may be possible to show a direct relationship between such a difficulty as cross laterality or short sight and reading or spelling disability: it is rarely possible to establish this in the majority of cases. There is, however, a small body of work which suggests that certain types of neurological damage may be correlated with specific educational failure, especially in arithmetic; and that in a

few cases severe disturbance of figure ground perception is causally related to reading disability.[1] So too a recent genetic study produces some evidence for a congenital and hereditary element in the causation of dyslexia.[2]

EMOTIONAL FACTORS

Clearly the process of learning, whether we approach it by factor analysis, by the analysis of errors made by children, by teaching method experiments or by the case study of individual children, is more complex and elusive than in our optimism we should like to think. Much of our difficulty lies in the fact that our studies, especially those involving large groups of children, are mainly cross-sectional. Only rarely can they take account of the fact that success and failure, adjustment and maladjustment are things of slow growth the root causes of which may have disappeared by the time we come to study the results.

The association between emotional disturbance and educational failure, noted by Burt and almost all subsequent investigators is relevant here and the connexion between educational difficulties and delinquency cannot be ignored. Yet even in this field direct casual relationships are difficult to establish, even in a particular, intensively studied case. By the time a child reaches the middle of his junior school career it is hard to say whether his retardation is caused by his emotional problems or whether failure in school is provoking more widespread social and emotional difficulties.

In this matter studies of children's adjustment and progress in the early years of schooling are of crucial importance. Cummings, in her survey of nursery, infant and junior school children

[1] MacMeeken, M., *Ocular Dominance in Relation to Developmental Aphasia.* University of London Press, 1939.

[2] Hallgren, B., *Specific Dyslexia.* Copenhagen: Ejnar Munksgaard, 1950. It is of interest to note here a possible hereditary element in arithmetic ability and disability. Cf. Burt, C., "Structure of the Mind," *British Journal of Educational Psychology*, 1949, xix, 186.

aged two to seven, reports that restlessness, lack of concentration, and excitability have a very high incidence, especially in the five to seven group, and her follow-up studies six and eighteen months later suggest that lack of concentration proves highly resistant to change. Here clearly is a seed-bed of retardation. Pringle's more intensively clinical study of 200 normal and 50 problem children between six and eight years old reveals the way in which failure and lack of adjustment in school are often only superficial symptoms of more widespread and cumulative misadjustment in the entire social and emotional development of the child and are related to environmental demands, particularly to the expectations of the home.[1] On the other hand, cases of educational retardation occur in children in whose background no adverse factors can be discovered and who show little emotional disturbance.

As the clinician sees it, broad generalizations concerning the causes of success and failure in learning the basic skills in the primary school cannot be made as yet; nor can we safely lay down diagnostic procedures for universal application. The only truly limiting factor in educational progress seems to be general educable capacity or intelligence. Social, physical and physiological factors, unless they are very extreme, are of importance only in so far as they are mirrored in the emotional life of the child himself. To a large extent the same can be said of teaching methods. These seem to be good if they enlist the child's emotional energy in the task of learning; and readiness to learn is a reflection of willingness to learn. It is as much bred of prior experience of success, self-confidence and the desire to move co-operatively forward as it is of intellectual or physiological maturation. Conversely the child who fails to move forward in school may be the victim of unsatisfied needs, deprived of

[1] See Cummings, J. D., "The Incidence of Emotional Symptoms in School Children". *British Journal of Educational Psychology*, 1944, xiv, 151–61; "A Follow-up Study of Emotional Symptoms in School Children". *British Journal of Educational Psychology*, 1946, xvi, 163–77; Kellmer-Pringle, M. L., "Social Maturity and Social Competence". *Educational Review*, 1951, iii, 113–28.

experience or shaken in his confidence by failures to adjust to his companions or to his own world of home.

This has important implications for education generally, for remedial work and for future research in these closely related fields. Our present techniques of case study are fumbling, clumsy, and lacking in objectivity. As yet few studies of the effects of education or remedial work have been planned on an adequate statistical and experimental basis to take account not merely of the measurable aspects of personality and attainment but also of development and of the dynamic interrelationships between the environment, the child's interpretation of it and the ways in which outside intervention will modify the entire emotional-social-intellectual gestalt. The often noted fact that similar results in clinics, remedial centres and schools are obtained by the most diverse methods leads us to suspect that the vital factor is the establishment between child and therapist, tutor or teacher of a secure personal relationship. It is to the exploration of this difficult field that future research should be directed.

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II. *Backwardness and Retardation*

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Valentine, H. B., "Some Results of Remedial Education in a Child Guidance Centre". *British Journal of Educational Psychology*, 1951, xxi, 145–9.

In their several ways, these three papers indicate recent methods of approach to the problems of retardation.

Burt, C. L., *The Backward Child.* University of London Press, 1937. The most authoritative piece of research work in the field and a classic of investigational technique.

Schonell, F. J., *Backwardness in the Basic Subjects.* Edinburgh: Oliver and Boyd, 1942.

Schonell, F. J., *Diagnosis of Individual Difficulties in Arithmetic.* Edinburgh: Oliver and Boyd, 1937. In a practical way these two books

cover the diagnosis and treatment of common difficulties in reading, spelling, oral and written English and arithmetic in the primary school.

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VII

CURRENT TRENDS IN CLINICAL PSYCHOLOGY

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HISTORICAL INTRODUCTION

THE term "Clinical Psychology" has not been long in use in Britain. Clinical Psychologists have functioned here for many years under other names: when engaged in work with children they have usually been known as Educational Psychologists. The new term has been borrowed from America where a flourishing profession of Clinical Psychologists is rapidly developing.

In America, at the end of the last century, Lightner Witmer opened a "Psychological Clinic" in the University of Pennsylvania. His plan was to study the child "as an individual whole" using mental and educational tests together with a biographical approach. The approach was to be made by a team of experts in the psychological, social, educational and medical fields. This work was further developed by Healy in his studies in delinquency, and by the National Committee for Mental Hygiene which was founded in 1909 to reform conditions in mental hospitals. About 1920 this Committee began to take an interest in what is now generally known as Child Guidance and, with financial aid from the Commonwealth Fund, began to organize and operate demonstration clinics to "develop the psychiatric study of delinquent and difficult children, to develop sound

methods of treatment, and to provide courses of training". The work in this field expanded but was not systematized, especially in the adult field, until, during the second world war, it became clear that about one-third of the young men in the armed forces were in need of the kind of help that had been given to children for some years past. Since the end of the war the activities of the Veterans' Administration have led to careful consideration of the functions and standards of training for Clinical Psychologists. Since 1940 there has been a rapid development in this field. Journals dealing with matters of interest to Clinical Psychologists have been published and now about 25 per cent of the members of American Psychological Association describe themselves as Clinical Psychologists.

In Britain, long before the foundation of Witmer's Psychological Clinic, academic psychologists took an interest in the psychology of individual differences. Sully and McDougall encouraged child study, and societies were set up to develop it. In the British tradition there was emphasis on the need for a combined theoretical and practical approach. The early developments centred round the Universities of London, Liverpool, and Oxford, but it soon became clear that if the work was to be really effective local centres would have to be established and the work of the psychologists would have to be integrated with education.

In 1913 Sir Cyril Burt was appointed Psychologist to the London County Council—"the first appointment of an official Child Psychologist in any civilized country" (Thorndike), and so became the first practising Clinical Psychologist in Britain to use a combined social, medical, psychological and educational approach.

In 1920 Crichton Miller founded the Institute of Medical Psychology, later known as the Tavistock Clinic, and from this the early psychiatric aspects of the work began to develop. The Commonwealth Fund, which had done so much to help the Child Guidance movement in America, agreed to provide

similar financial help here. The Child Guidance Council came into being and the London Child Guidance Clinic, the first Demonstration Clinic in England, was started in 1928 under the direction of Dr. William Moody and the personnel were trained in America. Psychologists trained in this tradition were concerned with problems of individual children and became known as Educational Psychologists.

Development of the Child Guidance Service was slow in the pre-war years, but by 1943 there were seventy Child Guidance Centres in England, Scotland, and Wales. The problems of evacuated children during the war emphasized the need for this type of service and the Education Act of 1944 has encouraged local authorities to set up Child Guidance services and employ Educational Psychologists. At the present time there are more than 155 Child Guidance units, and Educational Psychologists have advised local education authorities on many educational problems and helped a number of maladjusted children.

Psychologists working in the field of adult mental health have, from the first, been referred to as Clinical Psychologists. In the adult field there was little development until, during the war, it was demonstrated that psychologists and psychiatrists could work together with advantages to both disciplines and to the patient. In 1947 the Maudsley Hospital, Institute of Psychiatry, started the first training course. Since the Health Act of 1948 there has been a growing demand for the services of Clinical Psychologists in mental hospitals and psychiatric out-patient clinics, rehabilitation centres, and other branches of the medical service, for example, paediatricians, neurologists, neurosurgeons and physicians are making increasing use of the contributions of Clinical Psychologists in diagnosis and in planning the educational, occupational and social rehabilitation of their patients.

It would seem, therefore, that a new profession has been established. Standards of training must be set up and a clarification of function is needed. The British Psychological Society and the appropriate Whitley Council panel, co-operating with

medical and educational bodies, will have an important role to play in this. Though there is much to be learnt from the Americans, no doubt in Britain Clinical Psychology will develop along somewhat different lines in the next few years. There will be many experiments before there is complete clarification of function, standards, and training, but a beginning has been made.

FUNCTIONS OF CLINICAL PSYCHOLOGISTS

The nature of the work undertaken by a Clinical Psychologist is at present very varied and depends on the unit in which he works, and on his own interests.

Clinical Psychologists working with children, i.e., Educational Psychologists, are employed in the school psychological service of local educational authorities. Such a psychological service often includes a Child Guidance unit in which a psychologist co-operates with psychiatrists and psychiatric social workers. In the main, Educational Psychologists still seem to carry out the terms of reference which Professor Sir Cyril Burt received from the London County Council in 1913, viz.:

1. To investigate individual cases referred by teachers, parents, School Medical Officers, Care Committee Officers or magistrates, and make recommendations. (When carrying out this function the Educational Psychologist may be said to function as a Clinical Psychologist.)
2. From time to time to carry out regular surveys of the school population.
3. To advise the Education Committee on the psychological aspects of its problems and proposals, particularly in regard to the selection of pupils to the various types of school.
4. Where necessary to initiate and supervise research.

The Educational Psychologist in a Child Guidance Clinic is concerned in:

(*a*) Assessing the child's personality, his intellectual assets and defects and his attainments, and in throwing light on the causes of his difficulties.

(*b*) Some aspects of the treatment, e.g., remedial treatment of the children with special disabilities and their re-education through play.

(*c*) He acts as a link between the Clinic and the School, advising teachers on the handling of individual problem children.

In the wider field where he functions as adviser to the Chief Education Officer on psychological matters he is concerned with the organization of special schools and classes for the education of educationally subnormal, maladjusted, and physically handicapped children. He may have some part to play in the allocation of children at the age of eleven to different types of secondary education. His help is likely to be most valuable in the assessment of the borderline pupils where his special knowledge of individual children is likely to be an asset. He is also concerned in helping with the education of parents in principles of mental health and in the further education of teachers through lectures on backwardness and prevention of maladjustment.

Outside the school psychological service paediatricians and physicians and surgeons are asking psychologists to advise on social and educational needs of handicapped children, e.g., the deaf, blind, physically handicapped children with organic lesions, and those suffering from illnesses involving prolonged hospital treatment, like tuberculous meningitis, encephalitis, etc. The clinical psychologist is also asked to assist in researches where intellectual and personality changes may follow different types of illnesses and different forms of treatment.

Clinical Psychologists Working with Adults. Psychologists are employed in mental hospitals, in psychiatric out-patient clinics, in rehabilitation centres, and in general hospitals where they co-operate with neurologists, neurosurgeons, and physicians. They carry out four main functions:

1. *Diagnosis.* Psychologists assist psychiatrists in the diagnosis of psycho-neurotic and psychotic conditions.

(*a*) They are concerned in estimating the relation of a patient's intellectual level to his psychological difficulties: attention is paid to relative strengths and weaknesses rather than to global figures. Early signs of deterioration of intellect or personality may be disclosed by psychological examination before becoming clinically apparent.

(*b*) Outside the intellectual sphere some objective assessment can be made of the patient's personality as a whole, his interests and his attitude towards both his personal problems and varied aspects of his cultural milieu such as his religious denomination.

2. *Treatment.* The Psychologist does not engage in direct psychotherapy unless specially trained to do so, in which case he becomes a lay-psychotherapist rather than a Clinical Psychologist; but he has an indirect influence on treatment by helping to assess

(*a*) the likelihood of response to psychiatric treatment,

(*b*) the type of treatment most likely to succeed,

(*c*) some latent artistic, musical or literary bent which can be used constructively in treatment. Moreover, he can give a valuable objective estimation of the changes in intellect and personality resulting from treatment.

(*d*) In some cases the actual interviews with the psychologist may be of help to the patient in gaining a better understanding of himself, his true capacity and limitations revealed in his performance in the tests.

3. *Rehabilitation.* In the rehabilitation programme of any patient whether mentally or physically handicapped, the psychologist can play a role very similar to that of adviser upon educational rehabilitation in Child Guidance. He can ensure that the chosen occupation makes the most use of the patient's capacities and the least call upon his weaknesses, either intellectual or emotional: that the natural mechanic explores the inside of

cars rather than a ledger of figures; that the introvert designs rather than travels in vacuum cleaners.

4. *Research.* The psychologist's special statistical knowledge enables him to design experiments in the clinical field and analyse their results. Much psychological research is being carried out upon intellectual and personality changes resulting from such physical methods of treatment as psycho-surgery, insulin, E.C.T., and drugs. In co-operation with neurologists, neuro-surgeons and neuro-psychiatrists, psychological processes associated with specific cerebral mechanisms are being investigated. The results of some of these research projects will be discussed in detail in later chapters.

TEST TECHNIQUES

A well-known neurologist has remarked: "You can learn most about a person by watching him take a cat, a canary, and a suitcase on a journey to Skye". The average psychologist is unfortunately debarred from this ideal means of observing behaviour; an artificial situation, the testing interview, must therefore be substituted. Under such conditions a mere arbitrarily selected sample of all the abilities and traits can be explored. As Zangwill puts it in his *Introduction to Modern Psychology*: "The only final test is the demands of education, of occupation, and survival".

The techniques employed in this artificial situation are divided for the purpose of discussion into:

(1) tests of intellectual functions;
(2) tests of attitude and interest;
(3) tests involving projective techniques;
(4) tests involving psycho-physical responses to various stimuli.

1. *Tests of intellectual function.* Two parallel subdivisions of this battery of tests can be made:

(*a*) tests of group or individual type;
(*b*) tests estimating the general intelligence level or special abilities or disabilities.

Because people taking tests are of different personality structure and because the test situations make different demands upon them some people perform better on tests of the group type while others produce better results on individual tests. In a test of the group type the subject, once given instructions, does not receive further stimulus from, and there is no immediate rapport with, the examiner. In a test of the individual type the psychologist can encourage the subject to make an effort to complete difficult tasks; this fact helps subjects who lack drive and initiative, but some may find the situation difficult since it does involve rapport with another person. For these reasons it is desirable to use both group and individual types of tests if a full psychological investigation is required so that an estimate can be made of the subject's optimum performance as well as his level of functioning in what is, for him, the more difficult situation.

A general intelligence test gives some indication of the subject's level of functioning and the pattern of the test results, derived from the sub-test scores, suggests the presence of special strengths and weaknesses which can be investigated more fully by means of tests of special abilities and special intellectual functions. Numerous special cognitive functions are explored in a full clinical psychological investigation: among them may be mentioned tests of memory, perseveration, fluency, verbal expression, learning, concept formation, logical reasoning, musical, artistic and mechanical ability, and manual dexterity. It is often also necessary to examine the actual educational attainments of the subject.

2. *Tests of attitude and interest.* Here again the tests may be of a general type giving the pattern of the subject's attitudes and interests and covering many aspects of his personal life and cultural milieu, or they may be more specific and single out a particular attitude—towards such subjects as health or religion—for more detailed investigation. These tests are usually in the form of a questionnaire. Statements, which have to be classified into "true" or "false" or "cannot say" may be printed on

cards for sorting or in booklet form. Many patients find the card sorting method more interesting.

3. *Tests involving projective techniques.* Frank describes this method of test administration as one in which "the individual is presented with a plastic and unstructured situation to which he can give his own personal forms and meanings or manipulate or re-act to the situation in his own individual way".

Again these tests may attempt to assess many aspects of the personality or a single trait. They may be sub-divided according to the method of administration into controlled and uncontrolled tests. In controlled tests the subject is presented with a number of possible interpretations of the material and has to express a preference. In the uncontrolled type the subject is free to describe and interpret the material in his own way. While the scoring of the controlled type is more objective, it yields less information about the personality than the uncontrolled.

Projective tests may also be sub-divided according to the degree of structure in the material presented. For example, the material may be almost unstructured, like ink blots, or require the interpretation of a definite situation presented as a picture. Sometimes free construction may be involved, for example, tests where the subject draws, or assembles a mosaic, or completes a sentence.

These projective techniques have limitations, e.g., they are time-consuming, difficult to score and interpret, and they lack reliability and validity. However, when used in conjunction with other tests they often throw light on the discrepancies between the subject's ability and his capacity to use this constructively, as well as on many personality traits. They are particularly useful in measuring changes in the subject's power of self-expression over a period of time or during a course of treatment.

4. *Tests involving psycho-physical responses to various stimuli.* There is an increasing tendency to make use of tests of autonomic functioning since the results of these tests are less influenced than the previous three groups by changes in mood, attitude and

interest. Such tests are of flicker fusion, pain reactions, psychogalvanic reflexes, autonomic balance, suggestibility and dark adaptation.

In addition there are special test batteries for children or adults with severe handicaps such as blindness, deafness, cerebral palsy, etc.

Books listed in the bibliography describe and evaluate some of the tests most frequently used by clinical psychologists.

GENERAL METHODS OF APPROACH

The selection of tests depends upon:

(*a*) the age of the individual;
(*b*) his general level of intelligence;
(*c*) his education and social background;
(*d*) the specific problems which are to be investigated.

It will be clear that tests designed for the small child must differ radically from those for the adult since each must contain material which will be of sufficient interest to encourage the subject to make an effort. The standard and content of the tests must be related not only to the subject's level of intelligence but also to such environmental influences as education and upbringing.

Psychologists are required to contribute to the solution of many different clinical problems and must select an appropriate battery of tests. A battery of tests helpful in deciding on a programme of occupational rehabilitation would stress mainly special aptitudes and disabilities, while organic changes would be better reflected in tests of special cognitive functions, like memory, perseveration, and abstract thinking.

In the initial stages of this kind of investigation a general cognitive test, a general attitude test, and a general projective test are often given. Hypotheses about the subject's functioning can be based on these findings and later tested with more specific techniques.

The final interpretation of the test results must take into

account such influences as social background, the developmental and educational biography, and the occupational career. (Special traits such as the artist's and writer's powers of self-expression must be given due weight.)

Some psychologists tend to concentrate on the quantitative measurement of traits; others emphasize the qualitative value of the subject's performance in the tests, taking into consideration his behaviour, his method of solving problems and his introspections. All psychologists, however, would probably agree that the pattern of tests results is more important than global figures like intelligence quotients and there is an increasing tendency to use tests which easily yield such a pattern.

Despite the recognized limitations of test techniques—which research may slowly overcome—the contribution made by the Clinical Psychologist to the understanding of maladjustment in children or adults is increasingly appreciated: the collaboration of teachers, other educationists, neurologists, psychiatrists and psychologists is likely to benefit all their disciplines.

TRAINING

The pre-training qualifications and the content of training for Clinical Psychologists working with children and adults are outlined below.

In general, the qualifications for the Educational Psychologist working with children are:

1. An honours degree in psychology or a recognized equivalent, to provide the necessary academic background.

2. Teaching experience. This is regarded as important because psychologists have to

   (*a*) be accepted by teachers as colleagues;

   (*b*) have a practical knowledge of the educational system, since advice on placement cannot be realistic unless the person making the recommendation is aware of its practical implications;

(*c*) know what can be done for individual children within a large class and so give practical advice to teachers;

(*d*) need not only theoretical but practical knowledge of the behaviour of normal children of different ages in order to acquire the background necessary for the assessment of deviations from the normal.

3. Clinical training. This extends over one year and has both theoretical and practical objectives which have been developed over many years. A few points only can be mentioned:

(*a*) The student has to learn the art of approaching children individually whatever their age, degree of emotional disturbance or type of mental or physical handicap. Unless the psychologist can make satisfactory contact with the child and an appropriate adaptation to him during the testing interview, there is no assurance that the resulting assessment of the child's abilities or of the use he is making of these abilities will be sufficiently reliable to form a basis for recommendations.

(*b*) He has to learn to see the implications of his findings both for the child's individual development and for his social living.

(*c*) He has to learn to use the tests as diagnostic tools so that he not only measures the child's performance but gains also some insight into the mental processes involved. He has to learn how to report and interpret his findings or the findings of the team of which he is a member to others concerned with education and child welfare. In order to carry this out successfully he needs knowledge both of legislation affecting children and of the powers and responsibilities of other workers such as Children's Officers, Youth Employment Officers.

(*d*) He needs to co-operate with teachers both in the handling of individual maladjusted children and in the application of psychological principles in the classroom.

(*e*) He needs to be trained in methods of re-education and to

gain adequate knowledge of all forms of special educational treatment, for example the organization of special schools and classes.

(*f*) He should also supplement his knowledge of the general principles of psychology and educational research gained during the academic years.

There is agreement upon the desirability of extending training for a further year so that the student may work for that time in a clinic or centre under the supervision of a senior psychologist and a training department.

Training in the Adult Field. In this country there is not yet any training comparable with the four year course for Clinical Psychologists which has been developed in America. There students spend part of the time in a University Training Department and part as interns in a hospital or clinic. At present in Britain there is a training course of eleven months at the Maudsley Hospital, and under the National Health Service graduate psychologists will receive training by working for two years as assistant psychologists under the supervision of a senior. Doubtless the next few years will see further developments in training in which it is hoped the British Psychological Society will be intimately concerned. Finally, there may emerge a clearer picture of the functions of Clinical Psychologists and the training they require.

It would seem that the training of Clinical Psychologists is incomplete in some aspects unless they acquire experience working with both adults and children, since psychologists who have not worked with children tend to lack awareness of the importance of developmental problems, while those who do not investigate adult psychiatric problems may see less clearly where the maladjustments of childhood are leading and may fail to recognize among children the early signs of more serious abnormality. In the future there may be a common training for psychologists working with children or with adults and specialization may take place at a later stage.

It would appear that Clinical Psychology has become established as a profession and there is likely to be an increasing demand for the services of appropriately trained psychologists. In the first issue of the *Journal of Clinical Psychology*, in January 1945, the Editor said: "The future of Clinical Psychology is intimately related to the development of psychiatry, education, and other social sciences which concern themselves with the problems of human adjustment." This would certainly appear to be the case and neurology might well be added to the list of sciences with which Clinical Psychology is associated.

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VIII

OBJECTIVE PSYCHOLOGICAL STUDIES IN PSYCHIATRY

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In this paper I will review a number of objective psychological studies in psychiatry selected largely from work in progress at the Institute of Psychiatry (Maudsley Hospital). These studies have two main aims: first, the scientific description of the characteristics of persons who are mentally abnormal and, second, the study of the effects of treatment on persons who are mentally abnormal. Many and varied statistical (including factor analytic) and experimental methodologies have been used. Perhaps the chief characteristic common to all the studies is that they are "objective". By this I mean they are executed and reported in such a way that it would be possible for others to repeat the work.

THE MAIN DIMENSIONS OF PERSONALITY

One approach, which may be called the taxonomic, dimensional or factorial approach is seen in the work of H. J. Eysenck. The essential argument of this approach is that, before it is possible to describe the abnormal personality or to study the effects of treatment upon personality, it is necessary to determine objectively the ways in which human beings differ from one another. It is considered that in the early stage of the scientific study of personality the greatest need is for certain taxonomic

principles or principles of classification and that after these have been determined more detailed experiments may profitably be carried out. Large scale researches are devoted to the fulfilment of this aim, the methodological tool which has been found most useful being the statistical technique known as factorial analysis. This technique provides an objective criterion for determining whether traits postulated, on other grounds, exist and can be measured. The usual approach of the factorist is to design several tests to measure the postulated trait, to administer the tests to a large sample of persons and to submit the resulting intercorrelations to a factorial analysis. The factors extracted from this analysis are identified by the use of internal and external criteria and the factorist satisfies himself that it is meaningful to work with the concept he has isolated. The ultimate goal of the factorial method in the field covered by the title of this paper is to show along which dimensions human beings vary; it will then be possible to describe persons or the results of treatment in objective terms.

Such a goal is, however, far off; Eysenck's own programme, for instance, has been in progress for nearly a decade yet only factors of the broadest kind have so far been considered. A large number of research projects have been carried out ranging from small, individual projects to large team researches. The general plan of the researches has been to administer to many and varied samples, normal samples (industrial, from the Services, university students, children), neurotic patients and psychotic patients batteries of psychological tests: written questionnaires, objective performance tests and projective tests. Factorial analyses have been carried out on these data principally, at this stage, to demonstrate the existence of a trait or factor and to show which tests measure these factors most efficiently. The initial plotting of the dimensions along which human beings vary show that there are at least three of these factors: called neuroticism, psychoticism and introversion-extraversion, these being operationally defined in terms of psychological tests. These factors

are discussed elsewhere in this book by Dr. Himmelweit. A person's position in this factor space can be plotted according to his scores on the tests which define these factors. Clearly, a considerable amount of work remains to be done in defining and measuring already established factors and in discovering further factors. A start has, however, been made, that is, the field of personality has been initially mapped so that the objective description of groups and of the results of treatments is possible even if only in terms of two or three broad factors.

Passing from a consideration of studies whose aim has been principally to define the field we come now to studies the characteristics of which are either that the hypotheses investigated have been derived from the initial factorial studies or use is made of tests about whose content knowledge has been gained from these factorial researches.

PERSONALITY CHANGES FOLLOWING PREFRONTAL LEUCOTOMY

A. Petrie studied personality changes following prefrontal leucotomy, a cerebral operation carried out for therapeutic purposes in certain cases of neuroses and psychoses which prove intractable to less drastic methods of treatment. Previous research had shown no coherent pattern of changes following this operation. Petrie postulated that many of the clinically observable changes following prefrontal leucotomy could be described in terms of shifts on certain of the fundamental factors of personality, patients becoming, in terms of the tests defining these factors, less intelligent, less neurotic and more extraverted. She demonstrated these changes conclusively in a moderately sized group of severely neurotic patients using a large battery of intelligence tests and objective personality tests. Further, she has since shown that the tests are sensitive enough to demonstrate that personality changes following a less radical operative

procedure are milder than those following a standard prefrontal leucotomy but are in the same direction.

Another study based on the results of previous factor analytic studies is that of J. L. Standen,[1] working in the field of psychosomatic medicine. He tested the hypothesis that patients suffering from hyperthyroidism (thyrotoxicosis) are more neurotic and more introverted than a group of control patients. Thirty-one patients who had been treated for thyrotoxicosis and a group of control patients with no psychosomatic symptoms were tested with a battery of personality tests including written tests of neuroticism and objective tests such as body sway, level of aspiration, manual dexterity, sense of humour and persistence. Both hypotheses received support from the test results, evidence being provided in favour of the greater neuroticism and introversion of the thyrotoxic group.

PERSONALITY AND GASTRO-INTESTINAL DISORDERS

Another research, also in the field of psychosomatic medicine, was carried out by E. Poser,[2] as a preliminary check on a broad clinical hypothesis, namely that there is a specific emotional constellation in the personality associated with a specific kind of organic disorder. By means of a battery of psychological tests an attempt was made to show that significant differences exist in personality between two groups of patients with gastro-intestinal disorders. Thirty patients with duodenal ulcer and an equal number of patients with ulcerative colitis were investigated as well as a group of normal control subjects. The three groups were equated for age, sex, and performance on an intelligence test. A medical and psychiatric history was obtained from all patients. To date only the analysis of the objective test results has been completed. Two of these tests, namely, the body sway test of

[1] "An Experimental Study of a Psychosomatic Problem". Paper read to the British Psychological Society, 18th November, 1950.

[2] "An Experimental Investigation of Some Psychological Aspects of the Body-Mind Relation". Ph.D. Thesis, University of London, 1952.

suggestibility and a test of pain tolerance were found to differentiate significantly between the two experimental groups. High scores on the body sway test and low pain tolerance scores were associated with ulcerative colitis. The objective test results are consistent with the hypothesis that specific personality traits are common to patients suffering from the same organic disorder and that such a group can be differentiated from patients in another disease group by the nature and—or extent of these traits of personality.

NEUROTICISM AND HEREDITY

We come now to two studies the emphasis of which is different from that of the studies considered so far. These studies are primarily developmental studies, the first concerned particularly with hereditary considerations, the second with the effects of environment.

The first of these studies was carried out by H. J. Eyseneck and D. B. Prell. The topic under investigation was the inheritance of neuroticism. The research design was ingenious, use being made both of the so-called "twin method" and the method of factor analysis. This study is described in more detail by Dr. Himmelweit. Here I will only emphasize the importance of the suggestion that individual differences in neuroticism may, in large part, have an hereditary basis. Methodologically, the study is of interest as an attempt to establish the genetic basis of a statistical factor.

BREAST-FEEDING AND PERSONALITY

The second of these studies was carried out by F. Goldman, the subject of her investigation being the relation between breast-feeding and character formation. Description of adult character in terms of childhood experience is one of the basic principles of psychoanalytic characterology. Goldman's research is concerned with "oral" character traits and their origin. This involved two questions: first, it was necessary objectively to verify the psycho-

analytic description of oral character traits and syndromes. Nineteen traits were tested by means of rating scales using 115 adult subjects. Traits tested included Optimism, Pessimism, Sociability, Aloofness, Dependence, Ambition, and others. These scales had a satisfactory reliability. Using a factor analytic procedure, a bipolat type factor was found, highly saturated with positive and negative manifestations of omnipotence and narcissism, and characterized mainly by the traits of Pessimism, Passivity, Aloofness, Oral Aggression (verbal), Endocathexis and Autonomy at one end; and Optimism, Exocathexis, Nurturance, and Sociability at the opposite end of the trait distribution. The composition of this factor shows a striking correspondence to the oral character type as described in the psychoanalytic literature. The second question in this research was concerned with the etiology of the oral character traits and syndromes. Goldman investigated the hypothesis that oral pessimism (depression), as defined by a person's score on the type factor or pattern of oral pessimism or optimism, is related to time of weaning. A low but highly significant relationship was found between early weaning and oral pessimism. The results seem to indicate that length of breast-feeding is a significant factor in the etiology of oral pessimism and probably of depression, but, as the relationship is low there are other factors involved in oral pessimism which account for it to a greater extent and which still await investigation. This study is of importance as a contribution to the experimental verification of psychoanalytic hypotheses. Methodologically the study is of interest because it illustrates the use of the factor analytic technique, not in plotting a little known field, but in a carefully designed experiment for the verification of a specific hypothesis.

A STUDY OF PERCEPTION

I would now like to consider an entirely different approach to the experimental study of psychiatry. The study is by M. B.

Shapiro, on the experimental investigation of a perceptual anomaly. An initial observation was made that some patients doing the Koh's Block Test would make a model correctly but would misorientate it. Further experiments were carried out with a single patient, the position of the figure and ground being systematically varied using the knowledge from one experiment to suggest hypotheses for the next, in order to determine the precise conditions under which the anomaly would appear, that is, to bring the anomaly under experimental control. This being successfully accomplished, it was necessary to examine other patients who showed the anomaly to determine, if possible, the clinical concomitants of the anomaly. In the series of patients so far examined and compared with control subjects not showing the anomaly, persons showing the anomaly seem, in a high proportion of cases, to have a lesion of the brain; the test appears sensitive in picking out persons who, on other evidence, are known to have lesions of the basal ganglia. This study is of interest not only because, being purely an experimental study, it contrasts interestingly with the studies mentioned up to this point which are largely statistical. It is of additional interest because, whereas most experimental studies are, from the beginning, planned within a definite theoretical framework, Shapiro started with no theory, merely with the empirical observation of an anomaly of perception in an individual patient. He then attempted to account for the appearance and non-appearance of this phenomenon and decided to test certain precise hypotheses based on Goldstein and Scheerer's general hypothesis that perceptual anomalies are a function of figure ground disturbance. The ultimate importance of the work will depend either on how far he is successful in accounting for his experimental observations in terms of this or other current theories of perception, or from theories he develops to account for the phenomenon, how far additional phenomena of normal or abnormal perception can be accurately predicted. In any event, the work may be of practical importance in the production

of a test for detecting patients who have a certain type of subcortical brain lesion.

EFFECTS OF ELECTRO-CONVULSIVE THERAPY

Earlier in this paper, it was suggested that an important group of objective studies in psychiatry concerned the effects of psychiatric treatments. Physical methods of treatment, rather than psychological, are probably the most suitable for objective study largely on practical grounds: they do not take so long, so they can the more easily be followed up by single investigators; they are given to moderately large groups of patients at a time and, while patients suffering from psychiatric disorders can never be said to be truly homogenous, patients known to benefit from the three most widely used physical methods of treatment, and for whom these treatments are usually recommended, namely insulin and electric shock and prefrontal leucotomy, fall into fairly well defined clinical groupings. I have described earlier in this review an investigation on the effects of prefrontal leucotomy. I would now like to describe two studies on the effects of electric convulsive therapy. These studies provide an interesting contrast in their methods of attack on the same topic.

J. Callagan[1] studied the effects of electric convulsive therapy on performance on a number of personality tests, the cognitive tests including tests of abstraction, fluency and concentration and the performance tests including tests of expressive movements personal tempo, manual dexterity, persistence and suggestibility. Two groups of patients were matched for age, sex, intelligence, general physical health and severity of depression and an experimental design used where in the first part of the procedure one group was given treatment while the other acted as a control; later the controls were given treatment the others not. Both groups were re-tested twice, that is, after either group had

[1] "The Effect of Electric Convulsive Therapy on the Test Performance of Hospitalized Depressed Patients". Ph.D. Thesis, University of London, 1952.

received treatment. The results to date suggest that changes after treatment on tests of this type are few, the most interesting changes being a transitory decrease in score on a test of verbal fluency immediately after treatment which is recovered in five weeks and a considerable reduction in score on a test of manual dexterity. On tests of expressive movement, patients show an increase in expansiveness immediately after treatment. On a test of Oscillation (Necker Cube), speed of reversals tends to increase, this change being in the direction of greater normality.

J. Brengelmann investigated the effects of electric convulsive therapy on learning and memory.[1] In a previous research Brengelmann had studied learning and memory processes in normal University students. The material he used consisted of complex patterns as contrasted with the simple patterns used by most investigators in this field. The subjects were shown the patterns tachistoscopically for brief periods until these had been learned perfectly. The characteristics of the learning and retention curves were then studied, an important feature of the work being the attempt to score the responses as error levels instead of as dichotomous (wrong-right) answers. Individual differences are revealed in many cases by the method of scoring error levels which would not have been found or would have been shown to a much lesser degree when measured in terms of dichotomous scores (or number of reproductions). In the present study, Brengelmann applied this method of scoring a learning task to patients undergoing electric convulsive therapy. He found that, typically, after the first convulsion a test is performed easily but only at a very high error level and there is no improvement even after a large number of reproductions. After later treatments there is a tendency for the error level to drop considerably but the same patient may still be unable to reproduce the patterns correctly. Considerable individual differences were found between patients on such a learning task after the same number of shocks.

[1] Study to be submitted for the Ph.D. degree in the University of London.

In evaluating these two approaches to the scientific study of the effects of electric convulsive therapy, in favour of Callagan's large-scale statistical approach it must be pointed out that it adds to what has been done previously in two major respects: firstly, whereas previous research has been concerned chiefly with the effects of electric convulsive therapy on limited aspects of personality, such as memory, his investigation makes an approach to the study of wider facets of the personality. Secondly, the experimental design, involving the use of a control group, is a great improvement on any design used previously. Brengelmann's approach is of the greatest interest and importance in that he has been able to demonstrate experimentally that one of the immediate effects of a widely used physical method of treatment is to cause at least a temporary deterioration in learning ability.

The studies discussed in this paper may, I think, be considered typical of current objective psychological studies in psychiatry. These studies are interesting for a diversity both of problem tackled and of method used. Although the field has, as yet, been tapped superficially in many directions and investigated thoroughly in only a few, the experimental clinical psychologist has succeeded, I think, in supplying an impressive body of objective evidence to the psychiatrist.

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IX

SOCIAL CHANGE IN STRUCTURED GROUPS

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INTRODUCTION

THE purpose of this paper is to discuss examples of actual research, and to illustrate the scope and limitations of one approach to the study of social change in structured groups. By a structured group is meant a group with a manifest role system related to the task of the group. By social change is meant a manifest change in the role system or structure of the group, or in the group's definition of its task, or in its customary way of carrying out that task.

Three examples of work will be given. These are drawn from recent publications of the Tavistock Institute of Human Relations and may also indicate the nature of the technical link with the Tavistock Clinic, whose work is described in another chapter.

The *first* example is intended to illustrate the nature of the approach, the time-span of social change in the sense defined, and the inevitable relationship between participation and authority. The *second* describes the use of a participant approach in overcoming community resistance to social change. The *third* set of data refers not to social change but to social continuity, and is consistent with the theory that, under certain conditions, labour turnover is the resultant—in Lewinian terms—of a quasi-stationary process which acts as a self-regulating mechanism.

THE GLACIER PROJECT

The project from which this material is drawn may be briefly described as a technical collaboration between an Institute

research team and a light engineering factory of some fifteen hundred employees. The factory forms part of the Glacier Metal Company and we have come to describe the relationship as the Glacier Project. The purpose of the project—as agreed some three years ago and in a manner to be described—was to investigate the considerable gap which then existed between the declared aims and the actual achievement of the firm in relation to such participant practices as joint consultation. A study of the forces and mechanisms maintaining this gap, and of the efforts of the firm to close it, formed the main task of the project. After initial agreement between the factory management and the research team with regard to aims and methods of work, something over three months was spent in obtaining the agreement, after full explanation and discussion, of such highly relevant internal groups as the Works Council, the Supervisors Group and the Works Committee. This last was a workers' group closely linked to a number of local trade union organizations, and agreement on the project required reference upwards to the highest trade union levels.

The research policy suggested by the team—and ultimately accepted by all of the groups mentioned—was that of making the team's collaboration available to any group or individual who requested it. The team undertook to initiate no other investigation within the firm and to maintain strict professional confidence with respect to its relationships and discussions with any group. Only problems directly concerning that group would be considered, and only agreed communication would be made of anything which emerged or transpired. Such a policy involved agreement to publish, for example, only that material which had been fully worked-through with those concerned, and had in this way become past history which could be made public without embarrassment.

Requests for assistance were received from a number of groups within the factory; and something may be said, in condensed form, of the problems raised and the changes emerging. The

periods of time concerned were very considerable and must be related to the effort required to recognize and deal with latent factors in group behaviour.

In working with a group—say, at its regular weekly meetings—a member of the research team would in the main endeavour to help the group to recognize previously unrecognized components of its behaviour with respect to the problem under discussion; and, on such a basis, would offer for consideration interpretive hypotheses whose effect might be to permit a restructuring of the group's perception of its situation and hence a move towards more appropriate action.

The main result of such work, over two and a half years, was to facilitate the formulation and assimilation of a new company policy governing executive and consultative functions. In relation to this new policy, and in terms of gross change in social structure, these changes and the stage of work at which they occurred are indicated in Fig. 1. They were:

(*a*) restructuring of the Works Council from a two-sided worker-management council to a multi-group institution including representatives of workers and of all grades of staff;

(*b*) these changes in the Works Council were dependent on changes in the trade union sector of the factory. The workers' representative body (the Works Committee) had been composed of trade unionists who were not necessarily shop stewards; in the new situation this body has been replaced by a Shop Stewards Committee based on an increased union membership, sanctioned by the local union officials and fully linked to their organization;

(*c*) restructuring of the executive system from a loose network of heterogeneous groups into a hierarchy of chains made up of two-level nuclear commands. (A nuclear command is an executive with his immediate subordinates.);

(*d*) changeover from piece-rate to flat-rate method of payment in two main departments.

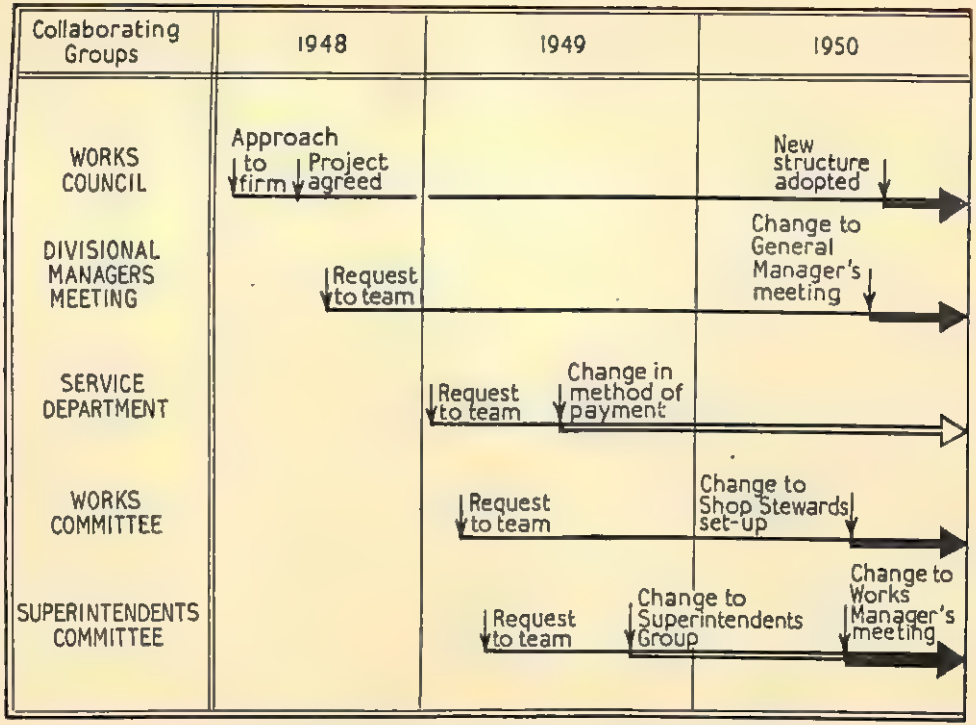

FIG. 1.—Course of Glacier Project: April 1948–December 1950

These changes were not—and, as previous experience has shown, could not with success have been—imposed by management; nor were they suggested by the research team. They emerged in large part from the slow and painful acquisition by groups and individuals of new perceptions of the significance of their behaviour, and may be summarized as results of a change in the customary way of tackling problems; that is, as a change in the culture of the factory.

This is perhaps most clearly seen in the abandoning of the original view amounting to an attitude that giving orders was authoritarian and therefore invariably to be rejected, and conversely, that participation in the form of joint consultation and group decision was invariably democratic. The view ultimately taken of such matters was within a different frame of reference—that of the participant *sanctioning* of authority and the responsible *carrying out* of executive roles. In work towards this change there were disclosed the existence and importance of serious anxiety among workers over effective participation in sanctioning of authority, and among executives over the personal and internal sanctioning required for the discharge of externally sanctioned authority. It also became obvious and accepted that the firm's early attempts at joint consultation had two different aspects: first, a constructive attempt to achieve a democratic culture and structure in the factory, and secondly, a collusive attempt to evade the anxieties mentioned above. The achievement of the new policy required the acceptable demonstration of the ineffective and anxiety-evading nature of many components in group behaviour by the examination of such behaviour in face-to-face situations.

It may be worth noting that these far-reaching changes, and the time, effort and disturbances connected with them, have in no way prevented the firm from making a considerable expansion but are regarded as a contribution to smooth growth and adaptation.

THE INITIATION OF A NEW FACTORY

In the outline now to be given it will be shown that an approach and hypotheses developed for Institute research purposes have been successfully employed by an industrial development executive, operating on his own so far as practical work was concerned. The opportunity to collaborate with him in an indirect way arose within a second major project of the Institute. This is concerned with a study of factors affecting the spread of new industrial techniques, and studies have been made of various problems of industrial training; of the working conference and the productivity team as methods of initiating and facilitating industrial change; and of some recent developments in underground organization in mining.

The problem with which we are here concerned was that of initiating a branch factory in a rather isolated Scottish community of 6,000 people. The process concerned was entirely new to the district, and required women workers who would have to acquire high personal skill in the operation of specialized machinery. The parent factory was located in England, and although the population and employment patterns of the Scottish community indicated the presence of adequate potential workers, the initial opinion of the project, given by regional authorities, was unfavourable. They felt that the attempt to found this branch factory would be no more likely to succeed than other attempts which, in comparable circumstances, had been spectacular failures. This pre-judgment was based on the strength of community memories of the high unemployment risks of branch factories, on community resistance to externally initiated schemes, and on experience of the operation of these factors in failure to obtain or train workers in comparable projects.

The development executive—who himself came from England—proceeded to explore these matters in face-to-face discussions, first with the officers of the local authorities and then with their elected members, and so provided an opportunity for

working through of anxieties and for testing of his integrity. In these discussions it began to emerge that many of the members of the authority were not only concerned over the threatening economic situation of their community, but as workers and parents were aware of the employment needs of their families. These positive interests emerged more strongly as the development executive described his plans, undertook to be replaced by local executives as soon as these could be found and trained, explained the position of the parent firm and agreed on their behalf that, if the initial project was successful, a larger and permanent factory would be built and maintained.

After discussion with local authority representatives he continued with a selected pattern of what, in Lewinian terms, would be called "gate-keepers"—that is, informal leaders or professional groups, and later of social groups and clubs. Although this exploratory process had increased the resistance of competing employers, he felt that sufficient positive interest had been mobilized in the community as a whole to justify the next step. Premises had been obtained and equipped. The training of workers in the complex skill required was known to be greatly affected by their anxiety level, and this in turn was linked not only to the strain of learning a difficult task, but to the fact that successful learning meant taking up a role in what the community still looked on as an alien group.

For reasons of this kind, training was initiated by selecting a group of six girls not only on a personality and aptitude basis but in part on a sociometric basis—that is, the formation of a training group of mutually acceptable individuals, whose cohesion, as a group, would have most chance of standing up to the strain of the training phase. The successful training of the first group greatly diminished anxiety and this was one factor in the improved learning curves of subsequent groups summarized in Fig. 2. The usual expectation of training phase output is much below even the lowest of the curves shown.

After some years, the weekly output of each worker is more

than double the Scottish average for the industry; it is also above the much higher English level. The same kind of relationship exists over the weekly earnings of the girls. The roles of manager, training officer and works manager, formerly carried

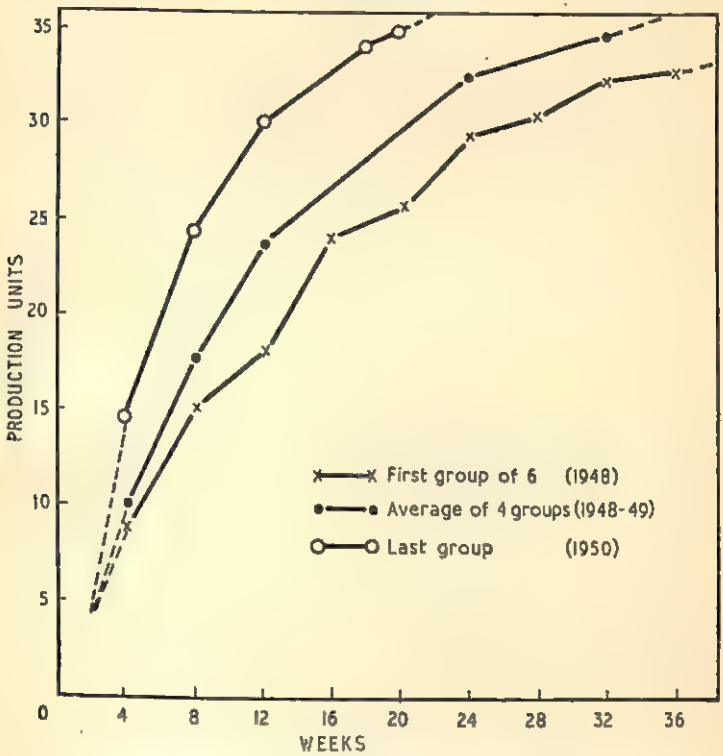

FIG. 2.—Comparative Output in Learning Phases of Small Working Groups in a New Organization, 1948–50

by the development executive, are now filled by trained local men and the new factory is in course of erection. The experience recorded has been repeated, with entirely comparable results, in two other European countries. It illustrates the potential value of discussion in facilitating social change; but it also offers the

opportunity to reflect on the probable size of the gap between actual and potential productivity in industry as a whole.

GROUP STABILITY AND LABOUR TURNOVER

Lastly, and in rather more detail than in the example just given, there will be described some findings which, if more widely confirmed and extended, may contribute to our understanding of processes by which structured groups maintain a certain consistency of behaviour, despite changes in size, composition and situation.

Before passing on to this, however, it should be recalled that our theme is the scope and limitations of the approach, outlined earlier, for the study of social change. Data from two factories will be given below, and it must be noted that one of these was the Glacier factory, with which the Institute has the special type of relationship described in the first example. This was not the case with the "X" factory, from which the contrast data were obtained; and, indeed, it need not have been the case with either factory, so long as no questions were raised as to the implications of the data or the need for more detailed study of an experimental character. The raising of any such questions, however, would directly and seriously concern the staff of any factory studying such matters, and it is open to question how far, in the circumstances outlined, such work could proceed without a responsible collaborative relationship. Questions of this kind are better answered on a basis of practical experience rather than of methodological theory; and we, at least, cannot so far pretend to that experience.

To return to the example, the points to be illustrated may be introduced by the trite but important observation that a structured group is made up of those who enter, stay, and, ultimately, leave. If the group is to persist through time those who leave must be replaced; and if the group is to maintain any unique institutional characteristics the existence is implied, in Lewinian

terms, of a quasi-stationary process which has the characteristics of a self-regulating mechanism.

Industrial studies of labour turnover have tended to be concerned with the rate at which employees leave a company, and with the reasons they give for leaving. The rate of entering and the reasons for entering have received less attention. The findings to be outlined were obtained by examining the total process by which an industrial institution both replaced its leavers and accommodated itself to the consequent change in the composition of its labour force. Attention was directed to the relationship between engagements and terminations rather than that between terminations and the number employed. In other words, the method was to follow up entrants rather than investigate leavers.

Three current aspects of labour-turnover phenomena were initially considered: first, the dominance, under full employment conditions, of employee request terminations; second, the short-term fluctuations of the numbers of those leaving as a function of the prevailing types of weekly engagement; and thirdly, longer term fluctuations in the numbers leaving as a reflection of the changing pattern of social and economic forces affecting particular firms. Consideration of factors likely to produce both of these types of fluctuations suggested the existence of a considerable and relatively constant residual. The actual fluctuations were less than would be explicable on grounds of the two types of factors indicated, had these alone been concerned. Accordingly, the concept was introduced of labour turnover as a process which is a function of the institution concerned and which will, therefore, have a particular form in a given factory. On this basis, an examination was made of the labour turnover of two factories with a changing overall size and composition. When the numbers of those who left in successive periods were calculated as percentages of the entrant group to which they belonged, the resulting curves—they could be called survival curves—conformed more and more closely to a regular pattern, the larger the entrant group became. The results are available in graphic form

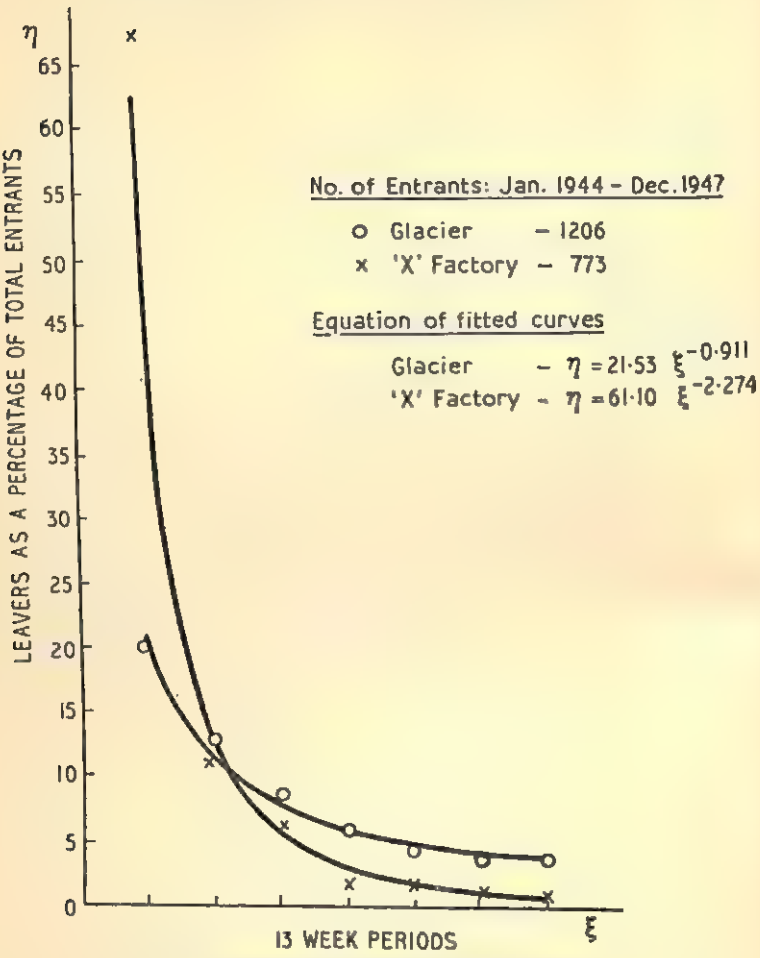

FIG. 3.—Glacier Metal Company and "X" Factory

(Fig. 3). It will be noted that these curves do not begin until the end of the first period. The fact that only a small proportion of entrants leave almost immediately and that engagements and terminations could very seldom happen simultaneously suggested that the distribution of frequencies shown concealed a mode in

the first three-month period. When the survival of a sufficiently large group of entrants could be examined so that the graph could be plotted for the first period, this mode could be demonstrated as occurring in the third week for Glacier, and the first week for "X" factory.

The general shape of these distributions suggested that the labour turnover process had three phases, representing three periods through which any group of entrants must pass:

(*a*) *The period of induction crisis*, during which a certain number of what may be called casualties results from the first mutual interaction between the engaging company and the entrant group.

(*b*) *The period of differential transit*, during which those who have survived induction learn the ways of life of the employing concern, and how far they have any place in it.

(*c*) *The period of settled connexion*, when those who have survived the first two periods take on a new character as quasi-permanent employees.

If the phenomena described are more widely confirmed—and there is some reason to believe this may be so—they will offer a method of approach which may be applicable to a wide range of institutions of interest to social psychologists.

One further development of this work may be briefly outlined. The method of representation of labour turnover described has been developed into an instrument whereby changes in the pattern of survival distribution could be detected as they occurred. By assuming that all who entered in any one month 'began to leave', so to speak, in the following month, the 'expected' leavers in any month can be calculated entirely from data about previous entrants. In the example to be given, the data went back to 1942 and enabled calculation of expected leavers from the Glacier factory for the years 1948, 1949 and 1950; and this led to the graph shown (Fig. 4). It should be said that during the first half of 1949 there was a period of redundancy and some 160 employees left at the company's request.

The graph—which both precedes and follows this period of redundancy—shows, first, that during the three years examined the rate of leaving has been such as to be consistent with the hypothesis that the survival pattern returned to its previous level after a period of disturbance; and secondly, that the redundancy period was preceded by a tendency for employees to stay in the

FIG. 4.—Simple and Cumulative Differences between Actual and Expected Leavers (Actual minus Expected)

firm for longer than they did in former periods. It would be unwise to say more of these results than that they are reconcilable with the theory that labour turnover is the resultant of a quasi-stationary process.

CONCLUSIONS

(*a*) Study of social process in reality situations, and in terms of the psychological and social forces concerned, raises problems of access for direct observation.

(*b*) Resistance to access may be balanced out by the need of a structured group for assistance in facilitating desired change, and the need for assistance may thus provide one set of circumstances in which access may become possible.

(*c*) In response to a request for technical collaboration in tackling a group problem, the research worker may take up a role whose existence and independence derive from its responsible professional character.

(*d*) Use of such roles may facilitate social change by the communication of insights derived from observations equally available to the group concerned and to the research worker.

(*e*) The illustrations given are intended to indicate the existence and importance of unacknowledged and unrecognized factors in group behaviour; the difficulty and the potentiality of providing or building conditions under which these factors can be acknowledged or recognized; and lastly, the complexity of the linked factors influencing social change in structured groups.

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X

THE SOCIAL PSYCHOLOGY OF EVERYDAY LIFE

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INTRODUCTION

This paper's title, borrowed with grateful acknowledgement, is that of a well-known article by Professor Hadley Cantril of Princeton University. Written seventeen years ago, it urged that really important events in the lives of ordinary people had seldom been studied by psychologists. We may surmise that this was because at that time the study of "popular" subjects was discreetly avoided by many social scientists, since it did not seem academically respectable. Six years later, the present writer, in an article "The 'Trivial' and 'Popular' in Psychology", surveyed some social problems studied by psychologists, and attempted to give psychological reasons for their preferences and avoidances.

To-day, one can report a considerable increase in the social significance of the subjects studied, especially if sharp distinctions are not drawn between the spheres of sociology, social psychology, anthropology, social medicine and psychiatry. The formation in 1950 of the British Sociological Association to link up these studies, and to encourage investigation into the social aspects of ecology, demography, economics, law and linguistics is an encouraging sign of progress here, while in the U.S.A. the establishment of Departments of Human Relations in universities speaks for itself.

The development of techniques for investigating attitudes and public opinion, for interviewing, e.g., in vocational guidance, medicine and anthropology, the study of likenesses and differences in national characteristics and the effect of social factors upon test-performances, the intensive study of patterns of culture, and tensions between communities, all these events and more, testify to a considerable change in the climate of opinion, as a glance at the chapter headings of Klineberg's or Krech and Crutchfield's books on Social Psychology will show. The ordinary man in his social setting here and now is gradually if slowly becoming an object of scientific interest.

SOME NEGLECTED TOPICS

There are still important subjects which have received relatively little attention from social psychologists. Of these the most obvious are peace and war, since to-day almost all problems of planning are affected by the possibility of another war. Another neglected subject in Britain is social stratification. To the study of these matters there certainly exist strong resistances; to discover and account for them is an interesting task for the social psychology of the future.

Until recently, social psychology was a study by a few members of the middle classes, of certain segments of middle-class behaviour. There is no social psychology of the British upper and working classes.

The fact that a subject is of obvious economic importance; e.g., fashions in dress, does not guarantee that it will be enthusiastically studied by economists, and acknowledgement that a subject is socially significant did not until recently, impel many sociologists to investigate it. There are, of course, fashions in all sciences. The remark once made about the subject of another section of the British Association for the Advancement of Science —that Universities tolerate its study so long as it has futility-value—is less true to-day; yet consideration of important aspects

of our social life are still left to the novelist or the writer of film-scenarios. (By one chance in a thousand, a psychologist may display skill in both these spheres.) The study of relations between human groups, which belongs to sociology, though it has obvious psychological implications, may attract a type of investigator who is happier when examining facts "objectively", as he claims. Yet this adverb begs several questions. A purely objective study of a human being's movements tells us, and indeed is meant to tell us, nothing about his inner experiences, emotions and desires—yet what can explain the great public interest taken in John Hersey's account of the effects upon a few persons of the bomb dropped on Hiroshima? A successful sociologist might be shy and apt to avoid personal relations, but any social psychology produced by such a person would lack not only warmth and intimacy, to use William James's phrase, but validity too.

Let us now consider some subjects which offer puzzling, important problems. One of the most urgent and obtrusive questions in the minds, not only of many administrators, politicians and philosophers, but of ordinary citizens, is whether we can escape a major war in the very near future. One might perhaps naïvely suppose that since 1918, psychologists, of all people, would have been attracted to study the causes of peace and war. Yet to-day few of our colleagues show any professional interest in these subjects. At the International Congress of Psychology held at Stockholm in July 1951, attended by over 600 psychologists, from nearly all countries, 187 papers were read. One only dealt directly with peace and war; two, very indirectly.

A fact of our civilization, interesting, particularly in England, to novelists, historians, politicians, and radio-play producers (but for it, how could some of these ply their trade?) is the presence of differences in social class, though "class" blurs other useful concepts, e.g., social strata and elites. At the Stockholm Congress only three papers dealt with class differences; two referred to England.

Differences in the manners and etiquette of various social groups bulk largely in travel-books or in post-holiday conversations, in which we describe minutely the queer goings-on of people in countries perhaps only a few miles from ours. Yet "superficial" differences in manners may be influential in causing friendship or enmity between nations. "Manners" themselves and the reasons, often cogent, for their existence, are seldom discussed, though often praised or blamed. A recent book, Ray Allister's[1] *Manners for Moderns*, full of valuable data, was scarcely noticed in our newspapers, though one described it useful for the "socially insecure".

RESISTANCES TO STUDYING IMPORTANT PROBLEMS

The aim of the present paper is not missionary, to persuade psychologists that vast fields of study are still neglected, but to suggest that discovering the causes of this oversight is an interesting psychological project. Consider differences in classes, strata and elites. Much has been written about inferiority complexes,[2] and this term has passed into popular use without much distortion of its meaning. In our socially stratified community many people suffer from real inferiority complexes, yet few psychologists have studied the relations of these quasi-pathological disturbances of thought and action to those attitudes of superiority and inferiority, which may be healthy and socially justifiable. Beyond doubt some members of this audience have worked in a hierarchy in which the presence of superior and inferior strata was unquestioningly accepted as necessary for functional efficiency. A member of the Army, in uniform, well drilled and conversant with his rights and duties, when addressed by a superior officer need not be troubled by an inferiority-complex or feel socially insecure, and may give orders, expecting to be obeyed, because of the acknowledged superiority of his own rank to that of some

[1] London: Convoy Publications, 1950.
[2] cf. Oliver Brachfeld, *Inferiority Feelings*. London: Routledge, 1951.

others. In certain countries, similar hierarchies outside the combatant Forces are accepted, in others resented and resisted, while in some communities many persons may not be certain if their attitude towards authority is directed by a sentiment or a complex. Our knowledge in this realm would be enriched by introspective accounts of how one feels in such "natural" situations of superiority or inferiority, and of the modifications of behaviour deliberately produced to adjust oneself. This would valuably supplement data about inferiority complexes.

Psycho-analysts have enriched our stock of concepts by that of intra-psychic "resistance". There seem to be special resistances to the task of studying certain problems from a social psychologist's standpoint.

In *The Boundaries of Science*,[1] Professor John MacMurray has discussed resistances to knowledge. We may class them as objective, existing in the physical world or in society, and subjective.

To students of the physical sciences, few external resistances are offered to-day, partly because of the century-long efforts of organizations like the British Association. Recently, indeed, some thinkers have expressed the wish that in certain sections of the physical sciences progress could be less headlong—a few would say Gadarene. In the trained scientist few inner resistances to studying the physical sciences exist, for he has been well-conditioned from a very early age.

Students of Zoology, Anatomy and Physiology still meet with outer resistances, partly financial, partly social: e.g., there are many varieties of objection to experiments upon animals and human beings. The problem of obtaining material for the teaching of anatomy once gave rise to differences of opinion in this city of Edinburgh. There are, too, subjective resistances—and "squeamishness" does not cover all these. Bitter, dogmatic controversies about birth-control, termination of pregnancy and artificial insemination still exist, and many social implications of

[1] London: Faber and Faber, 1939, pp. 53–71.

physiology, e.g., convulsive shock therapy, lobotomy and leucotomy, are matters for argument among scientists themselves.

To the study of psychology there are still many external resistances. As Professor MacMurray reminds us (I paraphrase) it cannot be assumed that because philosophers and writers concontinually refer to minds they would enthusiastically support the scientific study of mind. Resistance to psychology inside the ranks of the younger scientists at least, is far less than fifty years ago, when hardboiled scientists laughed at hypnotism and telepathy—which perhaps did not oblige them by ceasing to exist. It is relevant to mention that in this Association our own Section "J" began life as a sub-section of Physiology. When the Gallup Poll failed to predict Truman's election, while some of my non-scientific friends gleefully hailed this as the poll to end polls, scientists merely assumed that investigators would inquire why this particular one had gone wrong, and some saw, without being told, that an election result was never a fair test of such a poll. There are comparatively few external obstacles to the study of the psychology of the individual, so long as any social implications are not too obvious, as intelligence-testers have found in the last few years. Yet man shows a tiresome preference for living, when he can, in groups, and for thinking, feeling, behaving, even perceiving the external world in a manner influenced if not dictated by his social needs. Recognition of this fact has immensely complicated, and enriched, psychology in the last twenty years, as a glance at a summary of the rapidly increasing literature of "social perception" will show. Yet if it was for some psychologists a great mental jolt to pass from contemplating "colour-in-general", as treated by von Helmholtz and Hering, to considering the perception of "film-", "volume-" and "surface"-colours, as treated by David Katz, they have felt "socially insecure" (in another sense) when invited to regard the perception of coloured objects, in their frames of reference, as a social act. But to-day many young men are alive because,

being colour-blind, they were rejected as air pilots. As "mere" colours, which of course do not exist for the modern psychologist, the red and blue of the Brigade of Guards resemble those of the Salvation Army—for a Londoner at least the difference in perceiving their two flags may depend upon social more than upon spatial factors.

Consider the "sense of smell", as it used to be called. From Zwaardemaker to Coty is a distance too far for most psychologists' thoughts to travel, yet the *New Yorker's* "Profile", describing the factors in the rise of this industrialist and the influence of his great wealth upon French politics, offers rich material not only to the social psychologist but to the sociologist and the historian.

In our society, certain themes of obvious psychological significance are taboo—it is not my purpose here to consider possible reasons for this and whether they are "good" or "bad". The most obvious taboo is upon discussion of sex; though one must remember that in some people taboo-breaking may cause a fearful joy, perhaps childish, and that to join a powerful group, famed for success in taboo-smashing, may require little moral courage. Professor Gordon W. Allport has said that Freud succeeded in the almost impossible task of over-emphasizing the role of sex in western culture. He certainly gave us terms with which civilized people could discuss previously embarrassing subjects. To point out that there is still a taboo upon mentioning a person's social class may seem unnecessary, yet while some Marxians frequently emphasize "class", their opponents soft-pedal it.

Subjective causes of neglect by psychologists of certain subjects needing study are at present matter for speculation—often by reflective non-psychologists. One of the most cogent reasons for not noticing certain problems in social groups (in mentioning this I expose my professional colleagues to a newspaper's comment that psychologists at the British Association are particularly apt to be surprised by the obvious) is that in certain respects the

investigator is satisfied with things as they are. The busy farmer driving his cultivator at high speed in a Fenland field seldom bemoans the absence of mountains; few lawyers try to reform our laws; Marie Antoinette had little respect for theories of nutrition. Dissatisfaction felt intensely may distort even simple perception. Knowing this, even though it is nearly obvious, the psychologist, struggling to attain almost superhuman objectivity may feel himself open to censure as being a social reformer or "agitator"—so he leaves certain burning questions alone.

Such partly conscious, partly unconscious factors may cause the widespread lack of interest among psychologists in problems of peace and war and of social stratification. To inquire too closely into these may lay oneself open to the charge of "non-objectivity"—a fact which both Ibsen and Shaw noticed long ago in connexion with ethical problems of the medical practitioner.

It is time to realize frankly that in many people there are conscious reasons for not objecting to war. Unconscious causes exist too, but they have had a good Press for the last three decades, and more research into conscious causes is needed. Investigators who like a certain amount of resistance to push against can be assured of it in this sphere, since unconscious motives are easier to disavow, especially "humorously" (an effective defence in England), than conscious motives which may issue in behaviour observable by others.

Another reason for these scotomas may be the short-range success in the field of mental hygiene, of the concepts of social "adjustment" and maladjustment. Nietzsche's remark that the perfectly adapted organism is the parasite, is disturbing. Writers (usually in America) like George Murdoch, Stuart Chase and Norman Cousins remind us that it is high time for "social anthropologists" (and much social psychology is carried out under this title) to call public attention less to the differences between peoples and much more to their resemblance, even if this decreases, as it would, the "novelty-rarity" value of their

books and lectures, and therefore for the moment, their popular appeal. As Norman Cousins writes in *The Saturday Review of Literature*:[1]

> Now that education has served the purpose of instructing us about our variations, let it inform us about the things we have in common. Over and above the challenge represented by differences in skin pigmentation or the shape of our eyes or the colour of our hair or our cultural and religious and national backgrounds is a major and dominant challenge: the need to make this earth safe and fit for man. We may live in the two worlds of "East" and "West" but we have only one planet to do it in.

RECENT INVESTIGATIONS OF SOCIAL STRATIFICATION

Reflecting this growing interest in the social psychology of everyday life, research has been and is being carried on at Manchester into problems of conversation, the social and educational background of Members of Parliament and Her Majesty's Judges, impressions of personality and character from voice and speech, differences in personal attitude towards changes in one's own speech and that of others, social stratification and its affects on behaviour.

Among the most recent of these studies an investigation which is being carried out by Davide Boulting, C. E. M. Hansel and myself. Its subject is the tendency to associate voice and ways of speaking (this term includes choice of vocabulary, accent, intonation, speech-melody, etc.) with locality, occupation and social class.

Previous psychologists, including the present writer, have relied upon words, used by experimenters and subjects, to express judgements of personality. Comparatively little use has been made of the fact that in many countries (more particularly in England) judgements of personality from speech or facial

[1] 1951, vol. xxxiv, No. 31.

appearance are closely connected with impressions of the person's occupation and social class.

The present investigation uses as material, gramophone records of men reading two prescribed passages; one approved by phoneticians as bringing out many sounds expressing "geographical" dialect, the other designed to elicit class-differences in pronunciation ("social" dialect).

Of a number of speakers recorded, three were photographed similarly, with only their faces showing. Twelve comparable faces of men who had not taken part in the experiment were photographed, so that the face of a speaker could appear with four "catches". All directions are given visually by film-strips,[1] so as to prevent comparisons of the speech to be judged with that of the experimenters. Subjects, in making and recording their judgements, are not required to use or to think in words, since the speech is presented simultaneously with pictorial backgrounds not named, e.g., a map of the British Isles (Fig. 5), with numbered counties, so that the speaker's place of birth, and any place which subsequently has altered his speech (as judged by the subject) can be indicated by a number.

"Physical backgrounds" consist of specially prepared pictures showing men engaged in different work-activities, in a setting: e.g., rural, urban, industrial, at which the picture hints wordlessly (Pls. I, II). There are also "psychological" backgrounds e.g., in one, men are giving and taking orders in different situations. The separate pictures can be used for specific judgements; e.g., naval officer, broadcaster, boxer.

After judgements have been made, the subject is asked to guess which of five faces is the speaker's. He then sees all the backgrounds again, in inverse order, and may modify his judgements while seeing the "correct" face (Pl. III). About 60–100 people can participate simultaneously. The method avoids descriptive words, and any differences of interpretation which may be given them (e.g. "middle-class", "clerk").

[1] Made by S.I.F.A. (London) Ltd., 36 Southampton Street, London, W.C.2.

Pictures allow numerous interesting details to be seen and assimilated quickly. Many quick judgements are possible, and the

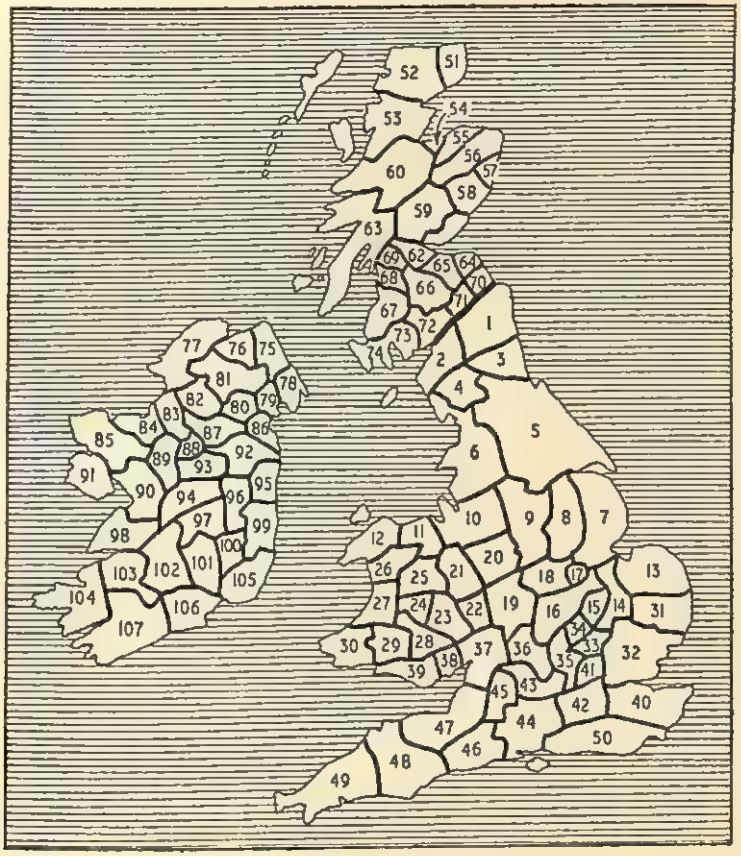

FIG. 5

film appeals to a young unsophisticated subject more than a questionnaire.

In some series a face is shown constantly against the various backgrounds, but in one series no face appears. Thus separate

variables (speech, face, backgrounds) can be kept constant, and the separate effect of each investigated. It is hoped that this investigation can be carried out in different parts of Great Britain and in several English-speaking countries, so that data of sociological interest may be obtained.

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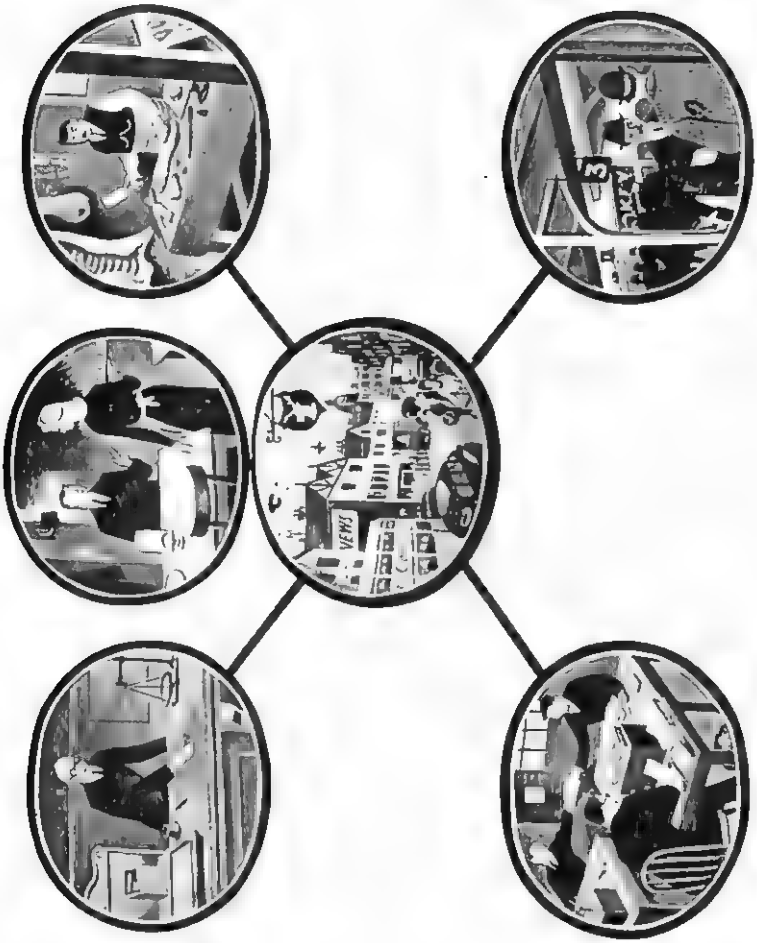
PLATE II



PART TWO

CONCEPTS AND METHODOLOGY

XI

CONTEMPORARY STUDIES OF MOTIVATION

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In this paper we shall not attempt to review past developments in the study of motivation. We shall restrict ourselves to the present, and single out what appear to us to be some of the more important present-day problems and trends in a field which is at once probably the most fertile and the least clearly defined in the whole of psychology. What we shall have to say will inevitably be selective and summary; and, with an eye on the titles of other chapters, we shall devote ourselves mainly to theoretical issues rather than to their practical applications.

RECENT VIEWS ON MOTIVATION

The fundamental problem, whether all behaviour is, in fact, motivated in some way, was, for many, settled by the Freudian doctrine of psychic determinism. All behaviour was motivated, and, if no conscious motive could be found, some unconscious motive must be operating. Modern psychology has now become somewhat more sceptical of the all-pervasive influence of uncon-

scious Freudian mechanisms, but, even so, the general concept of motivation is rarely questioned. Behaviour, it seems, always derives from some underlying personal driving force, directed towards some goal. Although these driving forces—for example, the need for food—are, of course, merely inferred from observed behaviour, their use in psychology can be quite legitimate, provided one can feel satisfied with something less than an ultimate neurological explanation.[1] But it is also true that used in this way, the concept of motivation can become relatively meaningless; and such extreme motives as, for example, Murray's "need to be active" or "need to be passive" seem to have little predictive value, and serve merely to maintain the assumption that, whether one does something or does nothing, whether one acts or not, one is none the less "motivated". Thus, Kluckhohn and Murray's attributing of "all forms of random, undirected, expressive, . . . behaviour" to a "need for activity"[2] seems to us dangerously like the old "faculty" type of explanation, and is certainly a far cry from McDougall's concept of a human motive as a striving towards some type of goal. Obviously, it is not difficult to postulate some need to explain any item of behaviour, provided one begins with the assumption that all behaviour *is* motivated. But is it?

Two recent contributions to this question, one by Maslow, the other by Norman Maier, are of particular interest. Maslow's argument,[3] briefly, is that many of the expressive and stylistic components of behaviour are unmotivated; that such behaviour as, for example, "the springy walk of the healthy person . . . the slumping posture of the depressed . . . the style of handwriting . . .", can be non-adaptive, have no aim or goal and in no way involve need-gratification. One may "need" to walk, to sit, or

[1] The dangers of divorcing "psychological" from "physiological" theories of motivation have, however, recently been stressed by Hebb. (See Hebb, D. O., *The Organisation of Behavior.* New York: Wiley & Sons, 1949, xi–xix.)

[2] Kluckhohn, C., and Murray, H. A. (eds.), *Personality in Nature, Society and Culture.* New York: Knopf, 1948, pp. 17–18.

[3] Maslow, A. H., "The Expressive Component of Behavior". *Psychological Review*, 1949, lvi, 261–72.

to write, but there is no need-satisfaction or goal-seeking necessarily involved in the particular mode of carrying out these activities. The distinction between adaptive and expressive behaviour was, of course, made by Allport and Vernon in their classic work on expressive movements,[1] but Maslow pushes the motivated-unmotivated distinction further, and obviously does not accept Allport and Vernon's implication that an act cannot be expressive without also being adaptive. Thus he leans on Goldstein's distinction between adaptive behaviour and the non-adaptive, catastrophic reactions that can occur in brain-injured cases, when adaptive, motivated behaviour is no longer possible. Maier's well-known work[2] relates to the peculiarly stereotyped and maladaptive responses that, as he has shown, can follow experimentally induced frustration. This was originally demonstrated with animal subjects, but has since been demonstrated also at the human level. Maier's claim is that, under frustrating and tension-producing conditions, goals that would normally influence behaviour may become quite ineffective, and that such behaviour must be explained, not in terms of particular goals being sought, but rather in terms of the nature of the instigating frustration. This thesis has obvious implications for diagnosis and therapy, and Maier is clearly opposed to the Freudian view that abnormal fixations "may serve as a relief for the patient". Fixations, he would say—and he extends this to certain forms of regressive and aggressive behaviour, and to certain phobias—are not primarily need-satisfying, adaptive, or goal-seeking. They are, to use his term, "frustration-instigated", and must be dealt with as such. Inferences regarding needs being satisfied, or goals being sought, can merely lead to diagnostic confusion. This line of argument has many subtleties, and it is possible that such behaviour as Maier has described can be as satisfactorily explained in more orthodox motivational terms—as, for example, by

[1] Allport, G. W. and Vernon, P. E., *Studies in Expressive Movement.* New York: MacMillan, 1933, p. 21.

[2] Maier, N. R. F., *Frustration: The Study of Behavior Without a Goal.* New York: McGraw-Hill, 1949.

Miller, as escape or avoidance behaviour, motivated by an acquired fear drive.[1] By choosing to ignore almost all contemporary research on learning except his own, Maier has probably made too much of his motivation-frustration dichotomy. Nevertheless, his experimental and theoretical approach to the study of fixated behaviour is stimulating; and, at the very least, he has, with Maslow and others, raised healthily awkward questions that any adequate theory of behaviour must ultimately answer.

MOTIVATION IN LEARNING

Within the field of learning, the role of motivation is perhaps the central problem, and certainly the one that has given rise to the bulk of animal studies over the past decade. As a later chapter is devoted to animal learning, we shall merely touch on the subject here. Briefly, the major controversial issues regarding motivation continue, in general, to be represented by the contrasting theories of Hull and Tolman. Hull, leaning heavily on Pavlovian principles of conditioning and Thorndike's Law of Effect, regards motivation as fundamentally a matter of need-reduction. Unless a response is followed by the reduction of some organic need-state—for example, hunger—or by some other stimulation previously associated with such need-reduction, the response will not be learned. Tolman, on the other hand, holds that, while learning due to need-reduction may occur, most learning, even at the level of the rat, is of a more "cognitive" kind, and, while such environmental stimuli as "rewards" and "punishments" in a learning situation may affect the animal's need-state, they act primarily as "signs", as informers, as spatial indicators of "what leads to what" in the course of the learning process. The position is, in fact, much more complicated than we have suggested, but essentially this is the crucial motivational

[1] See, for example, Miller, N. E., "Studies of Fear as an Acquirable Drive: I. Fear as Motivation and Fear-Reduction as Reinforcement in the Learning of New Responses". *Journal of Experimental Psychology*, 1948, xxxviii, 89–101.

issue on which have centred most of the theoretical elaborations necessary for the treatment of more complex behaviour.[1] The past decade has witnessed a mass of increasingly ingenious experiments, designed to settle this issue one way or the other; but the verdict on each theory is still "not proven". With interpretations of experimental results varying almost according to the particular theoretical sympathies of the experimenter, it is tempting to assume that the problem has become largely verbal. Hebb, however, in a recent attack on contemporary behaviour theorists for their inattention to the findings of neuro-physiology, has made it abundantly clear that this is by no means the case, and has challenged the whole concept of motivation as biological need-reduction.[2] The full impact of this on Hull's theory has yet to be seen.

Needless to say, doubts have been expressed concerning the applicability of such animal-based principles to complex problems of human behaviour: most directly and vehemently, perhaps, by Allport, in his notable "sign-symbol" address;[3] most frequently by those who simply see too unbridgeable a gap between physiological animal drives and complex human motives. This is not a problem we can deal with adequately here. But we would say this: First, such applications as have been made—for example, by Miller and Dollard to social learning[4], by Mowrer to problems of personality dynamics,[5] by Krech and Crutchfield to social psychology,[6] have been at least moderately successful. Secondly, the main dangers are those of confusing or extending too far the principles applied: thus Sargent's combining of Hull's

[1] For an appraisal of current theories of learning, see Hilgard, E. R., *Theories of Learning*. New York: Appleton-Century, 1948.

[2] Hebb, D. O., *op. cit.*, chapter 8.

[3] Allport, G. W. "Scientific Models and Human Morals". *Psychological Review*, 1947, liv, 182–92.

[4] Miller, N. E., and Dollard, J., *Social Learning and Imitation*. New Haven: Yale University Press, 1941.

[5] Mowrer, O. H., *Learning Theory and Personality Dynamics*. New York: Ronald Press, 1950, Part II.

[6] Krech, D., and Crutchfield, R. S., *Theory and Problems of Social Psychology*, New York: McGraw-Hill, 1948.

and Tolman's terminology has not been so successful,[1] and recent applications of Hullian theory to culture differences seem to us somewhat forced and premature. Thirdly, it may be, as Harlow's recent work on monkeys has indicated, that comparative studies of animal motives could be more profitably conducted on sub-human animals of a more advanced species than the rat. "Monkeys," Harlow says, "unlike rats, do not appear to be . . . familiar with modern motivational theory."[2]

At the human level, it is scarcely necessary to stress the importance, in learning and in the development and maintenance of skills, of such motivational influences as financial reward, praise, competition, prestige, target-setting, levels of aspiration, knowledge of results, and so on. But it is also true that, at the moment, we do not know precisely how such influences operate. And, when we turn to such subtle forms of motivation as "interest" and "curiosity", both recently reviewed by Berlyne,[3] the meagre state of our present knowledge becomes only too evident. One thing at least is certain, here no less than at the animal level, namely, that the role of motives in learning and skilled performance can never be fully defined and understood in terms of external incentives alone, without reference to the needs, wants and strivings of the learners and performers themselves.

MOTIVATION IN PERCEPTION

Motivation is linked to perception almost as closely as it is to learning; and here we want to say something of a particular approach to perception which has developed rapidly in the past three or four years and lays special emphasis on the importance of motives. This is the approach of Bruner, Postman, Klein, Brunswik and others, currently designated the "new look" in

[1] Sargent, S. S., *Social Psychology*. New York: Ronald Press 1950, chapter 5.
[2] Harlow, H. F., in Rohrer, J. H., and Sherif, M., *Social Psychology at the Crossroads*. New York: Harper, 1951, chapter 5.
[3] Berlyne, D. E., "Interest as a Psychological Concept". *British Journal of Psychology*, 1949, xxxix, 184–95; "Novelty and Curiosity as Determinants of Exploratory Behaviour". *British Journal of Psychology*, 1950, xli, 68–80.

perception. It developed from the already well-established fact that the nature of any percept depends, not only on the external stimulus and the resulting sensory state of the organism, but also on such factors as motives, predispositions and experience, which the perceiver brings to the perceptual situation. Bartlett had already shown the influence of such subjective factors in perceptions of simple stimuli,[1] Thouless that there were significant occupational differences in phenomenal regression,[2] and various other workers such as Sanford, Murray, Murphy, Levine and Sherif, that perceptual experience could be influenced by moods, hunger states, group opinions, rewards and punishments.[3] More recently, however, attempts have been made, not merely to confirm the fact that personal needs, emotions, and values do influence perception—as demonstrated in early experiments on judgements of sizes of coins, of German and American symbols, etc.—but rather to show just how, when, under what conditions, and to what degree, such influences operate.

In general, the experimental technique has been: (i) to use meaningful and usually complex stimuli, often of an emotionally toned or otherwise relevant nature; (ii) to present them tachistoscopically for brief and varying periods; (iii) to use, as perceivers, groups of subjects manifesting different needs, different intensities of the same need, different degrees of tension, or of satisfaction or deprivation; and (iv) to relate these subjective factors to perceptual differences in speed of recognizing the stimuli, and in the kinds of pre-recognition attempts or guesses made by the subjects. During the last three years, numerous experiments using this or a slightly varied technique have been carried out, and some of the provisional findings are these: by McClelland and Liberman, that the speed of recognizing words

[1] Bartlett, F. C., *Remembering*. Cambridge: Cambridge University Press, 1932, chapter 2.

[2] Thouless, R. H., "Individual Differences in Phenomenal Regression". *British Journal of Psychology*, 1932, xxii, 216–41.

[3] For summary references to these findings, see: Murphy, G., *Personality*. New York: Harper, 1947.

relating to achievement varies significantly, though indirectly, with independent ratings of the subjects' "need for achievement"; by McGinnies, that significantly greater galvanic-skin-responses are given to apparently unrecognized emotional words, than to apparently unrecognized neutral ones; by Vanderplas and Blake, that individual personal values are significantly related to the ease with which words bearing these values are recognized—thus confirming in the auditory field what had already been found with visual stimuli; by Bruner and Postman, that induced experimental stress results in less efficient recognition, failure to benefit from practice, and general "perceptual recklessness"; and by the same workers, that perceptual efficiency can be significantly impaired when the perceiver has a multiple "set"—that is, when he is set to look for more than one possible *kind* of stimulus. Moreover, it has been found that the nature of subjects' incorrect guesses about what they have seen tends also to be related to the motivational states, value systems, and other variables under study.[1]

Inevitably, such work has been criticized, in respect both of method (for example, over-reliance on the tachistoscope, and on the Allport-Vernon Study of Values) and of a certain theoretical bias in the interpretation of results.[2] But the bulk of the findings have survived, and have produced certain explanatory principles,[2] which, though provisional, nevertheless do represent some advance in this field.

What is likely to be the significance of these findings for psychology generally? In the first place, there is the obvious

[1] The original journal sources of these and other studies of functional perception are too numerous to cite here. Interested readers will find the bulk of this work discussed in: Blake, R. R., and Ramsey, G. V. (eds.), *Perception—An Approach to Personality*. New York: Ronald Press, 1951.

[2] See, for example: Pastore, N., "Need as a Determinant of Perception". *Journal of Psychology*, 1949, xxviii, 457–75; and Luchins, A. S., "An Evaluation of some Current Criticisms of Gestalt Psychological Work on Perception". *Psychological Review*, 1951, lviii, 69–95.

[3] For a summarized account of some of these principles, see: Bruner, J. S., and Postman L., in Dennis, W. (ed.), *Current Trends in Social Psychology*. Pittsburgh: University of Pittsburgh Press, 1948, pp. 71–118.

implication that theories of perception couched largely in terms of stimulus dimensions, structural organization, figure-ground relations, *prägnanz*, etc., while valid as far as they go, are normally incapable of explaining the diverse perceptions that arise from the same stimulus, when motivational and emotional factors are deliberately varied, instead of being held constant throughout an experimental group in the traditional manner. It is not merely that we need general laws of perception which will make allowances for individual differences: we also need general principles which will explain how these differences come about; and at the moment we have practically none. Secondly, such work clearly opens up a wide and relatively unexplored field for personality research that lies somewhere between, at the one extreme, studies of variations in so-called sensory functioning (such as Eysenck's work on dark-adaptation in neurotics, hysterics, and dysthymics,[1] and the Slaters' on auditory and visual acuity in neurotics)[2] and, at the other extreme, studies of responses in so-called projective techniques. It is a common fallacy that mere stimulus description, as distinct from associative elaboration, has little "projective" significance in these procedures. In fact, the presentation of specially selected, complex stimulus material under conditions that make correct description or judgement difficult, offers considerable scope for the study of what Klein has called "perceptual attitudes".[3] Frenkel-Brunswik's "intolerance of ambiguity" in rigid personalities is one recent example.[4] Klein's dimension of "levelling-sharpening" is another.

MOTIVATION IN SOCIAL PSYCHOLOGY

To turn to motivation in so-called social behaviour, we find contemporary psychology still grappling to some extent with

[1] Eysenck, H. J., *Dimensions of Personality*, London: Kegan Paul, 1947, pp.97–101.
[2] Slater, E. and Slater, P., "A Heuristic Thoeory of Neurosis", *Journal of Neurology, Neurosurgery and Psychiatry*, 1944, vii, 49–55.
[3] Klein, G. S., in Blake and Ramsey, *op. cit.*, chapter 12.
[4] Frenkel-Brunswik, E., "Intolerance of Ambiguity as an Emotional and Perceptual Personality Variable". *Journal of Personality*, 1949, xviii, 108–43.

the problem of individual *versus* social analysis of motives. In fact, there still seem to be two social psychologies: one trying vigorously to extend basic principles of so-called individual psychology to social and group behaviour; the other convinced that hope lies only in formulating fresh principles of a more sociological kind, sophisticated enough to cope with the complexities of social behaviour as it actually exists—and, by implication, this means "not as it is generally represented in the laboratory". Many do not regard this situation as unsatisfactory: they accept the fact that different explanatory concepts are valuable at different "levels" of analysis. Thus, it is sometimes held that we can explain inter-group conflict either in terms of the individual motives of the group members, or in terms of the contrast in their class structure. This may be true, provided that, in using such terms as "class", or, in other contexts, "group morale", "prejudice", "the Common Man", "culture pattern", etc., we do not assume that, simply by doing so, we have somehow overcome the need to concern ourselves with the individual motives involved in the situations under consideration. Krech and Crutchfield's attempt to construct a systematic social psychology on principles of general psychology[1] seems to us particularly timely at a period when many social psychologists appear almost to have forsaken their parent discipline for the fields of sociology and anthropology. Also of relevance here are Stagner's analyses of situations of industrial conflict in terms of individual motives and frustrations;[2] the attempts to assess the effects of radio communication in terms of individual gratifications;[3] the various efforts to interpret expressed attitudes in more functional, need-satisfying terms;[4] or, again, the increasing tendency to search for

[1] Krech and Crutchfield, *op. cit.*

[2] Stagner, R., "Psychological Aspects of Industrial Conflict: I. Perception". *Personnel Psychology*, 1948, i, 131–43.

[3] See, for example, Warner, W. L. and Henry, W. E., "The Radio Day-time Serial: A Symbolic Analysis". *Genetic Psychology Monographs*, 1948, xxxvii, 3–71.

[4] See, for example, the Harvard clinical study of attitudes toward Russia: Smith, M. B., Bruner, J. S. and White, R. W., "A Group Research Project on Dynamics and Measurement of Opinion". *International Journal of Opinion and Attitude Research*, 1947, i, 78–82.

basic affective processes beneath such terms as "stereotype", "prestige", etc. This, of course, does not mean that psychology is in any sense returning to the bad old pre-Lewinian days, when individuals were studied experimentally in a kind of social and cultural vacuum. Nor, of course, does it imply that complex social problems are readily explicable in simple motivational terms—an unfortunate present-day fallacy. It merely suggests we hope, a renewed desire to reconcile high-level with low-level concepts of analysis, and to ward off that split between general and social psychology with which we seem at the moment to be threatened.

From a host of other noticeable trends in this field, we might mention the following: the importance of adopted social roles—for example, the "life-and-soul-of-the-party" role, or the "heavy father" role—and the additional motives and frustrations which such role-taking involves; the emphasis in studies of leadership on the needs and other motives of potential followers, and not merely on the personal characteristics of the leader himself; or, again, the re-emergence of ego psychology within a social setting, with its distinction between need-frustration as threat to the personality, and need-blockage as mere deprivation.

OTHER PROBLEMS OF MOTIVATION THEORY

In this paper we have said nothing about certain problems which many may feel central to any discussion of motivation. We have not discussed the assessment of motives; we have merely touched on the subject of unconscious motivation; we have skirted the fundamental motivational problems involved in the development and organization of personality; and at many points we may appear to have been talking less about motivation as a separate process than of its relation to behaviour in general, and to the processes of learning and perception in particular. On this latter point, it does seem to us that, during the past decade, one of the main trends in psychological experiment and

theory has, in fact, been this increasing recognition of the interdependence of the long-separate fields of motivation and cognition. Current indications of this trend far exceed the few instances we have given in this paper, and are elsewhere evident in the increasing impact of learning theory on therapeutic practice;[1] in MacLeod's "phenomenology";[2] in studies of particular types of perceptual and cognitive breakdown in the so-called "functional" disorders;[3] in Frenkel-Brunswik's attempts to bring psycho-analysis into line with perception theory;[4] and so on. It may indeed be that psychology is on the brink of a new era of theory and experiment in which the arbitrarily distinguished concepts of motivation, perception and learning will give place to a more meaningful, and empirically useful, terminology.[5] This remains to be seen. Meanwhile it is certain that, in the next few decades, psychology has much to gain if the inter-relatedness of these three fields of study is increasingly stressed; it has much to lose if separate "psychologies" of motivation, perception, and learning are allowed to develop independently.

FURTHER READING

Of the books and articles cited above, those by Murphy (1947), Hilgard (1948), Krech and Crutchfield (1948), Maier (1949), Hebb (1949), Mowrer (1950), and Rohrer and Sherif (1951), are probably most suitable for general reference. Valuable material on motivation is also to be found in the following texts:

[1] See, for example, Shoben, E. J., "Psychotherapy as a Problem in Learning Theory". *Psychological Bulletin*, 1949, xlvi, 366–92; and Hilgard, E. R., "Human Motives and the Concept of the Self". *American Psychologist*, 1949, iv, 374–82.

[2] MacLeod, R. B., "The Phenomenological Approach to Social Psychology". *Psychological Review*, 1947, liv, 193–210. See also MacLeod, R. B., in Rohrer and Sherif, *op. cit.*, chapter 9.

[3] For a striking example of such disorganization in concept formation, see: Cameron, N., "Schizophrenic Thinking in a Problem-solving Situation". *Journal of Mental Science*, 1939, lxxxv, 1012–35.

[4] In Blake and Ramsey, *op. cit.*, chapter 13.

[5] As one pointer to such a theoretical revision, see Krech, D., "Notes toward a Psychological Theory". *Journal of Personality*, 1949, xviii, 66–87.

Freud, S., *New Introductory Lectures in Psychoanalysis.* New York: Norton, 1933.

Allport, G. W., *Personality.* New York: Holt, 1937.

Murray, H. A., *et al.*, *Explorations in Personality.* New York: Oxford University Press, 1938.

Frenkel-Brunswik, E., "Motivation and Behaviour". *Genetic Psychology Monographs*, 1942, xxvi, 121–265.

Young, P. T., *Emotions in Man and Animal.* New York: Wiley, 1943.

Hunt, J. McV. (ed.), *Personality and the Behavior Disorders.* New York: Ronald Press, 1944.

Cameron, N., *The Psychology of Behavior Disorders: A Biosocial Interpretation.* Boston: Houghton Mifflin, 1947.

MacKinnon, D. W., "Motivation", in Boring, E. G., Langfeld, H. S. and Weld, H. P. (eds.), *Foundations of Psychology.* New York: Wiley, 1948, 112–37.

Adorno, T. W., Frenkel-Brunswik, E., Levinson, D. J. and Sanford, R. N., *The Authoritarian Personality.* New York: Harper, 1950.

XII

THE PLACE OF EXPERIMENT IN PSYCHOLOGY

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INTRODUCTION

It has been a pretty general reproach to psychology that its experiments have fallen into one of two equally lamentable classes. The one consists of precise, controlled, often quantitative investigations whose results, it is said, are trivial, uninteresting and sometimes not even psychological. In the other class we find attacks upon problems which common sense, backed sometimes by expediency and sometimes by human feeling, suggests are vital and important. But these latter investigations, it is alleged, never bear proper scientific scrutiny and their results are apt later to reveal a deplorable lack of reliability. So far as past history is concerned this reproach is more than a little justified. The question is: what present indications are there that it can be avoided in future?

There is one bone in this contention which may be disposed of straightaway. Many people feel that experimental analysis *could* not, in the nature of the case, contribute anything to our understanding of the rich and varied facts of personal conscious experience and of individual behaviour. But must not the external physical world have presented as rich, as varied, and as inescapably immediate an appearance to the contemporaries of the early physicists? What, they might well have asked, could the behaviour of a stone dropped from a tower have to do with

the impressive vagaries of wind and water, with the changing play of colours on rock and tree, and with the unforeseen earthquake? Physics can now answer many detailed questions about thunderstorms *in general*, but can say little or nothing about the sequence of events in any *particular* storm. It may be a long time before the psychologist is in even so favourable a position as the physicist. But he ought not to be deterred from experimental enquiry because it does not—perhaps cannot—help directly to unravel the puzzles about human nature which most compel our interest.

MEASUREMENT IN PSYCHOLOGY

It is a common, and not entirely unfounded, belief that experiment must involve measurement, and it is another that nothing of a strictly psychological character can be measured. In consequence it is often felt that whenever we seek to measure in psychology we are bound to relapse into some other discipline, such as physiology. Quantification, however, does not in itself involve measurement, and some notable advances are to be expected of methods which correctly treat data obtained by *counting instances*. It is not my business, nor within my competence, to rehearse the triumphs already scored by statistical psychology in general. But I should like to point to one recent line of development which may bear closely upon future experimental methods. All use of statistics is dependent upon the possibility of calculating the random distribution of cases or values resulting from a given hypothesis. That is, we find out, either rigorously or approximately, what distributions *could* occur by chance and consider the data actually found in the light of this knowledge. Almost always, in the past, such calculation has involved a direct assumption that, underlying the situation considered, there is a set of equally probable possibilities. But it is fairly plain that, for psychological purposes, such a procedure may possess serious limitations. It is a characteristic of psychological situations that at any given moment a choice of possible

actions presents itself to the individual, and that this choice, and the probability of any given subsequent sequence of actions, is dependent upon the sequence of past choices. Such is the case in maze-running and in a number of games, of which chess is an example too complex for analysis. More generally, the notion of *transition-probability* and of *stochastic processes* is applicable to the everyday life of everybody. It is a notion which has gained considerable significance in recent physics, and the mathematical analysis of such chains of contingencies has latterly made very notable progress since the original work of Markoff some forty years back. A beginning of its application to experimental psychology has been made by Jonckheere[1] in relation to animal behaviour, by Whitfield[2] in regard to certain games which, though simple, involve memory and thought, and by Shannon in connexion with language and communication. Development from these promising beginnings may open up entirely fresh possibilities in experimental psychology.

The question whether anything psychological can be *measured*, however, has been wrangled about ever since the publication of Fechner's *Elemente der Psychophysik* in 1859. The British Association, indeed, kept a committee sitting upon it for nearly twenty years. The principal point at issue has, of course, been the measurement of sensation. This is a hoary problem, but brief reference to recent work may help to define the conditions of bringing measurement to bear upon psychological problems in general.

For the sake of brevity I will hazard agreement with all those distinguished people who have said that it is strictly nonsense to speak of measuring sensation. Measurement of physical magnitudes alone is possible, because measurement, as an actual practice, involves the properties of material bodies. But I do not think this means that, with ingenuity, we cannot use the measure-

[1] Reported at a meeting of the Experimental Psychology Group, 22nd July, 1950.

[2] To be published.

ment of the material aspects of a situation in which an individual is placed to reveal the nature of the processes which underlie his behaviour and experience.

Suppose, for instance, that we present alternately to a subject two pure tones of the same frequency. The intensity of one is kept constant, while that of the other is varied until the subject agrees that it sounds half as loud as the first. It is found that such judgements can in fact be made with the necessary degree of reliability. Next, taking as standard a tone whose intensity is equal to that of the second or variable tone of the first pair, we repeat the experiment and find a third value of intensity which is judged to sound half as loud as the second—and so on. We now plot, as abscissae, the values of intensity we have successively obtained against the numbers 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, etc., as ordinates. Such experiments have been carried out, for instance by Geiger and Firestone. A plot of values obtained in such an experiment is shown in Fig. 6. We thus obtain a *function* of the stimulus-intensity which takes values in accordance with the judgements of the subjects in respect of loudness. That is, if a sound of intensity I_1 sounds N times as loud as a sound of intensity I_2, then will $F(I_1) = N.F(I_2)$. It may be objected that we are not justified in the implied interpolation to give values of F(I) between those for which it was experimentally determined. And it would certainly be foolish to suggest that a person could judge one sound to be, say, 0·638 as loud as another. But we can repeat the experiment, starting with another original standard intensity, and provided we allow this first point to lie on the curve already obtained, the rest do so. It is, further, interesting to note that if the subject's instructions are to make judgements of "*one tenth as loud as*", he is able to comply, and a very similar function is obtained.

Personally I doubt whether, as has been suggested by Stevens and others, we are setting up a scale of *sensation-magnitude* by a procedure such as this. The curve obtained is that of a function of a physical magnitude, defined by a certain mode of organic

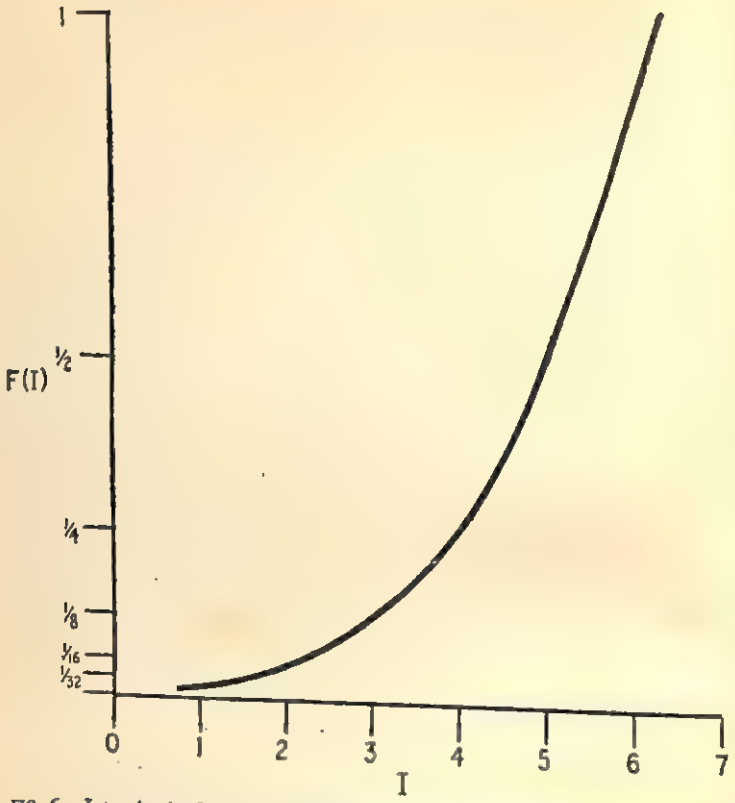

FIG. 6.—Intensity is plotted in logarithmic units relative to an arbitrary reference value. Frequency of pure tone is 1000 cycles per sec.
(*Redrawn from data in S. S. Stevens and H. Davis "Hearing"*)

reaction. But we are, on the other hand, obtaining some quantitative idea of the mechanisms underlying loudness judgements, and possibly (this remains to be confirmed) of the more general processes of response to sound stimuli. The *form* of the function obtained cannot be deduced from any external physical considerations. It is a property of the responding organism.

It may be that this particular psycho-physical function is not

of any great direct interest, although study of the manner in which it can change with changes in the condition and situation of the organism might be. But the way in which it is derived may serve to illustrate one aspect of the general problem of quantification in psychology. As Bartlett and others have pointed out, the human organism, unlike many simple physical systems, does not respond to a smooth variation of its physical environment with a smooth response. To small external changes it makes no response at all—it manifests a "threshold-characteristic"—possesses an "indifference-range". To larger changes its output consists in a series of *judgements*, or *discrete acts*. The work of Craik, Vince, Hick and others at Cambridge suggests that this is true even of skilled motor acts where the response is *apparently* smoothly variable. In this latter case the appearance of continuous output is produced by the physical properties of the muscular and skeletal systems involved. Thus our problem is to derive variables applicable to psychological mechanisms, not by direct measurement, but from the values of physical variables which produce given successions of judgements or other discrete acts. It will not matter if these variables are arbitrary provided: (i) they are derived by reliable, repeatable experiment, and (ii) a limited number of them allow us progressively to build up a picture of the mechanisms involved. If psychology demands quantification, as I think it ultimately must, this may in fact have to take a rather tiresome and indirect, but none the less definable, form. To procedures of this kind the physicist would be reduced only for the investigation of complex man-made machines embodying discontinuous servo-mechanisms, whose insides he is not allowed to inspect. Perhaps physiology and neuro-surgery may yet release the psychologist from this last handicap.

EXPERIMENTATION ON COMPLEX BEHAVIOUR

Another part of the general reproach we started by considering has to do with the fact that attempts to secure a simple experimen-

tal approach too often involve artificial and misleading fragmentation of the behaviour or experience of the individual experimented upon. Measurement of reaction times tells us little about actual skilled behaviour such as motor-driving. Learning nonsense syllables throws but a faint light on the problems of memory. But signs are appearing in at least two fields—those of skilled behaviour and of visual perception—that experimental method is moving out of this phase.

At Cambridge, under the guidance of Sir Frederic Bartlett, work has been going on for a number of years upon skilled acts, such as aiming a weapon, or flying an aeroplane, which may be regarded as calling on the resources of the individual as a whole. The resulting behavioural data in these cases might, indeed, be too complicated for analysis were it not for certain guiding principles offered by cybernetics, some of which were originally developed by the late Kenneth Craik, of Edinburgh and Cambridge, with just these problems in mind. These allow the aspects of performance which are relevant to possible theories of the mechanism to be picked out from the total data. Recent great improvements in instrumental resources allow these aspects to be recorded.

Another experimental device which allows us to avoid some of the pitfalls of fragmentation, and yet obtain relatively simple data, consists in causing the subject to make a series of discriminations repeated in time in such a way that each judgement or discriminatory act itself gives rise to the conditions which demand another. The physical change in respect of which the discrimination is made may itself be simple, but the continued activity represents the kind of adjustment individuals are engaged in in everyday life. Perhaps the simplest application of this principle is one we are developing at Reading. Here the subject's task is confined to that of pressing a key, whenever, and for as long as, he perceives a given change in his environment—such as a fluctuation in the intensity of a sound which was previously constant. The effect of his pressing the key is to reverse the

physical process which gave rise to the change, so that he soon ceases to perceive it. He then releases the key, and the process reverses itself again—and so on. The subject is thus made to act as a discontinuous control element in a cyclical system, much as the thermostat in a refrigerator operates, and the resulting equilibrium position may be continuously recorded in terms of the relevant physical variable. Changes in the equilibrium resulting from changes of environmental or inner organic conditions may then be investigated, and some light thrown on the sensory and adjustive processes involved.

But perhaps the most interesting feature of the methods for the study of skill developed at Cambridge and elsewhere (especially in the United States) lies in the possibility of applying them to the study of conditions such as fatigue, anxiety and old age. Thus Drew and Russell Davis have traced the changes of skill mechanisms produced by long-continued instrument flying and the latter has attempted some analysis of the disorders of skill which result from anxiety. More recently Welford and his associates have followed some of the changes in skilled function which occur with ageing. It may be that from these beginnings means will be found for a profitable attack on problems such as those of temperament, and of disorders at the psychological level, whether resulting from functional or organic disease.

EXPERIMENTATION IN THE FIELD OF PERCEPTION

Lastly, in the field of visual perception, there is some welcome evidence that it is becoming possible to avoid fragmentation and premature analysis, and yet select quantitative aspects for study. The experimental work of the first four decades of this century, in the hands of Wertheimer, Koffka, Thouless, Katz and many others, clearly demonstrated that the perceptual effect of any spatially restricted part of a total stimulus field is quite largely a function of the total field itself, and is not wholly determined by factors inherent in the restricted region. Important as this

conclusion is, however, it is negative in as much as the contribution of the total field could only be expressed in the most general way. Such unsatisfactory concepts as "setting", "framework", "pattern" and "context" were pressed into service, but could not directly lend themselves to any quantitative formulation.

Recent work in the United States, largely inspired by the pioneer experiments of Ames,[1] suggests that this difficulty may, in certain respects, be overcome. Supposing that we look at a normal room with one eye. So far as the geometrical characteristics of the stimulus situation are concerned, it could be replaced by any one of an infinite set of other, "distorted", rooms whose projections on the retina are the same, but which physically contain no vertical or horizontal elements, no right angles, and whose walls might even not be plane. A simple case which illustrates the principle is shown in Fig. 7. Ames and his collaborators have found that, provided care is taken that all the distortions are concordant, including those of doors, windows, pictures on the walls and patterns on the floor, the room is still seen as in "normal" right-angled form. There are two things that might be said about this. The first is that there may be certain features about the stimuli afforded by the "distorted" room, such as the gradient of the texture on the walls and the illumination of the various parts, which may not, as do the geometrical features, provide the same retinal stimuli as those provided by the "normal" room. We may say that the two stimulus situations are *equivalent* in some respects (for instance in regard to those geometrical properties which are involved in linear perspective), and *non-equivalent* in others, such as surface texture and illumination properties. Ames's demonstration shows that it is possible to devise experimental situations in which equivalences are prepotent over non-equivalences. But we must not forget that an important part of normal perceptual activity

[1] It is unfortunate that Ames has personally published so little of his work in an easily available form. A brief introductory account, by E. R. Hilgard, may be found in: Blake, R. R. & Ramsey, G. V. (edit.), *Perception—An Approach to Personality*, chapter 4, New York: Ronald Press, 1951.

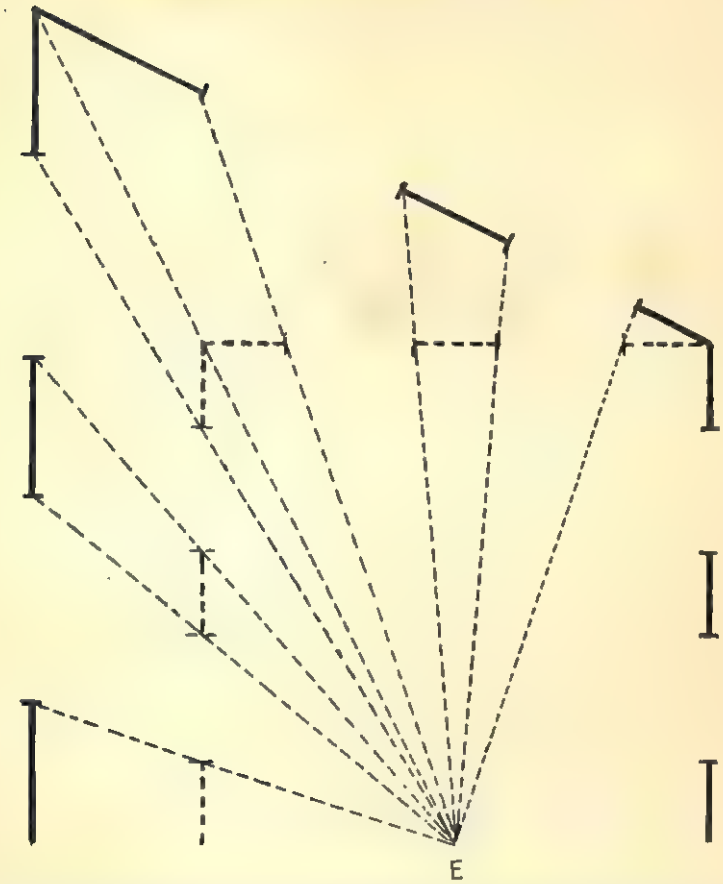

FIG. 7.—Plan of a simple Ames Equivalent Room. When viewed from the point E, the physical, distorted room (shown in heavy line) projects into the square one (broken line) and is perceived as such. An idea of the geometry of the projection may be had by considering the windows

is devoted to the *resolution of equivalences*. If, for instance, in looking at the "distorted" room, I move my eye to a fresh point of view, or open my second eye, the "normal" and the "distorted" rooms are no longer equivalent in respect of their geometrical properties.

The second thing that *might* be said about the equivalence of the "normal" and the "distorted" room is this. Why should the subject, looking at the "distorted" room, see anything but a normal right-angled one, since there is nothing in his retinal image, leaving aside small and unimportant non-equivalences such as those of texture, to tell him that he is not looking at a "normal" room? But this question, of course, invites the further one: Why when he is looking at a "normal" room does he not see a "distorted" one, since the argument about the retinal image will apply with equal force? It would seem that the "normal" form is, psychologically, a privileged one, whether for reasons connected with the subject's past experience, or because of tendencies inherent in his constitution. This suggests something which is, perhaps, absolutely basic to the theory of perception: namely that perception, at least of the degree of elaboration that we know in vision, is primarily dependent upon the operation of transformation mechanisms which treat the incoming signals in accordance with a general geometrical principle. We only tend to believe that the right-angled character of a perceived room is determined by the incoming signals because of the great frequency with which measurement and other checks confirm our impression of its geometrical properties. But this circumstance arises chiefly in our habit of building predominantly right-angled structures. To investigate the mechanisms of perception we must step outside this convenient but misleading convention.

About the operation in perception of general geometrical principles, there are three further points of importance to note. the first is that the relationships between different possible principles, which may be in operation at any given moment, are mathematically definable, and expectations can be developed for experimental test. The second is that the operation of any given principle does not apply only to the structural elements of the room, or whatever else is regarded, but is effective throughout the space contained in the room, and even to that seen through its windows. This is indicated by the experiment of placing

normal objects within an Ames distorted room. These suffer startling alterations of shape and size, which become even more impressive when they are moved about. The very operation of the geometrical principle which causes us to see a physically distorted room as a normal-shaped one provokes changes in everyday objects which seem to defy their material rigidity. Thirdly, it must not be supposed that the principle which treats, or tries to treat, all incoming signals in terms of right-angled structures is the only, or the ultimate one. It would seem, in fact, that our perceptual processes endeavour to "make sense" of the mass of stimuli which impinge on our sense-organs, by organizing the signals in accordance with one, or another, out of a repertoire of principles. Upon what ultimate criterion the actual choice of principle is made is a matter for further enquiry, but in general terms it may well be supposed that factors such as simplicity, economy, and expectation based on past experience, play a part. Work is in active progress on a number of these problems, not only in the United States but also in this country. Langdon, at Oxford, using string treated with fluorescent dye and illuminated with ultra-violet light, has devised a means of making distorted frameworks at once more flexible and less heroic than the trans-atlantic expedient of building rooms.

I have devoted some space to the discussion of the work of Ames and his followers because, it seems to me, it opens up truly fresh possibilities in the study of perception. Unlike most of his predecessors, Ames is less concerned with the way in which we perceive particular objects than with the general conditions which enable us to perceive in an orderly fashion at all. And he has paid particular attention to those geometrical conditions which offer hope of precise treatment. Doubtless future work will bring change in the interpretation of his experiments. But the beginning is exciting, not least because it suggests extension beyond the immediate field of perception. It happens sometimes in everyday life, and it can be easily arranged in the laboratory, that the stimulus situation is such that no single simple geometrical

or kinematic principle will allow consistent treatment of all the incoming signals. In such a case two consequences are found to follow. In the first place, the visual world takes on strikingly *unreal* appearance. In the second the individual may suffer a sense of loss of direct personal participation in it. His personal security may deteriorate, and in extreme cases there may be an experience of depersonalization. Now these are all phenomena which are commonplace in the field of clinical neuropsychiatry. If experiments which have their origin in the study of normal perception can be elaborated so as to make it possible to produce and study in a controlled form known personality disturbances, psychology will have a solid and significant contribution to make to the medical sciences.

I have picked up only a few fragments from a wide and growing field, and the choice has been a personal one. If I have emphasized quantitative work in the laboratory at the expense of other kinds of experiment, which do not involve measurement, or which are carried out in the clinic, the factory or the school, this is because the potentialities of the former are perhaps less widely known and recognized than they deserve. Nor have I wanted to suggest that experiment is sufficient. Psychology requires the highest possible standards in experiment: it also demands alert observation, ideas from other fields and, above all, insight.

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XIII

STATISTICAL ANALYSIS IN EDUCATIONAL PSYCHOLOGY

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CRITICISMS OF STATISTICAL ANALYSIS

THE use of statistical procedures in educational psychology has recently become the target for a good deal of criticism. Most of the arguments are of the semi-popular kind, which readily appeal to those non-mathematical readers who, whether teachers, students of psychology, or members of the general public, are apt to feel a little impatient over the pedantic and technical devices of the statistical investigator. Indeed, the kind of objections usually raised show a suggestive similarity with the criticisms put forward by earlier writers who protested against the introduction of mathematical methods into the older sciences—physics, chemistry, economics, and genetics, for example—where such procedures are now firmly established. Swift ridiculed Newton's calculations in astronomy: everyone remembers the scientific tailor in Laputa who took Gulliver's altitude by a quadrant, and measured his girth with other astronomical instruments and at length produced a suit of clothes, "quite out of shape", owing to a slip in his arithmetical calculations. Sir Humphry Davy criticized Lavoisier's quantitative

innovations in chemistry. The doctors laughed at the proposals of Laplace, Bentham, and Chadwick to use statistical procedures for studying the causes of epidemics and testing the efficiency of different cures. And now, in much the same spirit, we find some psychologists such as E. G. Chambers and O. L. Zangwill poking fun at the "psychometrists" who "treat love as a commodity which can be weighed and made up into parcels, tied with pink ribbon".

In educational psychology as elsewhere the same fundamental objection is urged again and again, though in slightly different forms. Qualitative characteristics, we are assured, cannot properly be treated by quantitative methods. A feeling of joy, the taste of ice-cream, the "pictures of fancy and the pleasures of hope"—these are things which it is impossible to weigh or measure, like so many pounds of flesh or so many pints of milk. The very nature of mental phenomena, it is said, is such that ordinary arithmetic cannot be applied to them; psychological contents and psychological processes are "never unidimensional entities: they form qualitative patterns, configurations, or *Gestalten*; and no one can measure a pattern". "To apply mathematical analysis to a living individual is to disrupt the total personality into measurable bits, and still hope that he will remain individual and alive". In short, the psychologist's proposal to draw up a table of test-measurements for a human personality is, so they seem to say, as fantastic as Olivia's proposal to "give out schedules of her beauty, . . . inventoried with every particle labelled: as, item, two lips, indifferent red; item, two grey eyes, with lids to them; item, one neck, one chin, and so forth".

Arguments and attacks along these lines were repeatedly put forward during the long drawn "Battle between Intuitionists and Psychometrists", as Spearman termed it; and they have been recently revived and amplified in some detail first by Chambers in an oft-quoted article in the *British Journal of Psychology*, then by Zangwill in his recent book on *Modern Psychology* in the Home

Study series, and finally by numerous medical and clinical psychologists who for some reason or other seem to suppose that metrical methods and case-history methods cannot be combined. The contentions of such writers, partly because of their very *naïveté*, are likely to have a wide appeal. Hence it seems urgently desirable that psychologists and educationists who habitually use statistical procedures should explicitly answer these criticisms, and defend, in terms equally clear, the research procedures on which they rely.

In this paper our aim will be very briefly to deal, in a simple and non-technical way, with three main issues that these criticisms raise. First, the theoretical problem: is the use of statistical procedures in educational psychology theoretically valid? Secondly, the practical problem: is the use of such procedures of any practical value? And finally, if the answer to both these questions turns out to be yes, we must then consider how far it is possible to improve or modify existing methods, so that whatever elements of truth may underlie current criticisms may be adequately faced or forestalled.

THE VALIDITY OF STATISTICAL METHODS IN EDUCATIONAL RESEARCH

The first question is a wide and highly technical subject. Here, therefore, we cannot examine it in any great detail. Our critics usually sum up their complaints by saying that "mathematical psychologists build elegant and dizzy numerical edifices, forgetting in their architectural zeal the flimsy foundations upon which their fabrics stand". Such a statement might well suggest that statistical psychologists have never really considered the essential prerequisites of numerical measurement, whether in mental science or in any other branch, and that if they did consider them, they would immediately find that mental phenomena fail to meet those requirements. Now, in point of fact, there are a

number of articles and books, written by various psychologists, in which they have set down quite clearly what are the recognized conditions for measurement of any kind; and they have shown, or claimed to show, that, over a very broad region at any rate, psychological data seem to satisfy such conditions quite as well as any other data. Burt, for example, adopting the standpoint of modern mathematics, has attempted to formulate in some detail the "postulates of mental measurement", and has endeavoured to show that many psychological phenomena are in approximate conformity with these demands. The most recent discussion, and an admirable one, is to be found in Stevens's chapter on "Measurement, Mathematics and Psychophysics" in the new *Handbook of Experimental Psychology*; those who prefer an elementary account, bearing more directly on educational problems, will find it in the volume on *Educational Measurement*, edited by Lindquist. Thus it is quite untrue to say that mathematical psychologists have "forgotten" the foundations on which their procedures rest.

However, even if their analyses had failed, even if our opponents were correct in supposing that no psychological characteristics could be subjected to measurement, that would still not prove that statistical procedures were inapplicable. The idea that statistics deal with nothing but measurable characteristics is a widespread but mistaken fallacy. Such critics have only to turn up any statistical text-book—Udny Yule's, for example—to discover that, while the second part deals with "quantitative" variables, measured by "numbers on a linear scale", the first part is concerned with "qualitative" attributes and their frequencies. Procedures of the latter kind seem to be continually overlooked by psychiatrists and others at child guidance clinics, who assure us that from the very nature of the case, their opinions on the causes of this or that child's misbehaviour can never be statistically checked, and the efficacy of their treatment can never be formally demonstrated, because "the variables concerned are qualities not quantities". Emotional characteris-

tics like anger or joy, and neurotic states like hysteria or anxiety, cannot, so they contend, be measured.

Now, even if we admitted that this was true, we should still be able to count up the number of children who are suffering from a given type of psychoneurosis, or the frequency with which Tom or Jessie exhibits such and such a symptom, or gives this or that response, or the percentage of occasions on which they responded to some trifling frustration by bad temper rather than by tears or a grin. By way of illustration let us take a concrete example. There was quite recently a heated discussion in the daily press as to whether the broadcast adventures of Dick Barton had a harmful effect on the children who listened to the programme. Are not the hair-raising escapades of this hero, we were asked, bound to frighten the young listener, and as a result induce worries, nightmares, and lack of sleep?

Questions like these are ordinarily discussed on the basis of private impressions or personal views. But such a method can hardly ever be conclusive. We need objective evidence; and, to claim any cogency, such evidence must be statistical. Accordingly, with the help of a research student, Miss Diana Scott, one of us carried out a small inquiry among a hundred boys between the ages of nine and twelve from the East End of London. No attempt was made to measure their emotional reactions on any linear scale. Instead they were asked a series of questions about the different wireless programmes they listened to, and about which they liked best; about their favourite books and comic papers; about the films they had seen; and so on. In particular, they were asked if they had ever been frightened by Dick Barton, and whether they could remember having had a bad dream after some story they might have read, or after some incident they might have seen on the screen or heard on the wireless. The frequency with which different replies were given could then be counted up, and subjected to a simple statistical analysis; and on these numerical data the final conclusions were based.

Now obviously some allowance must be made for possible inaccuracies in the children's replies. For example, we asked, among other things, which of their possessions they would most hate to lose. The list was headed by favourite cats and dogs, and their other choices ranged from football boots and bicycles to "My Mum" and somebody's goldfish. There was even one wag who had to put "my trousers". However, after freely discounting the inevitable defects of such a rough experimental procedure, certain conclusions stood out in clear relief. The most unexpected was that Dick Barton was by no means the popular hero that had commonly been supposed. When compared with the *Eagle*, the Saturday morning film club, the variety shows in the Light Programme, and serial thrillers intended for adult audiences, the Barton instalments sank rather low in the scale of popularity. Another and perhaps more important finding was that far more boys reported bad dreams after watching films with "A" certificates or hearing wireless programmes meant for grown-ups, than after listening to Dick Barton in the children's programme. At the cinema, "murders" were usually mentioned as the most frightening type of film; next came films about ghosts and then such stories as "Frankenstein", "The Spiral Staircase", and "Dracula's Daughter". Among wireless programmes, "Paul Temple" easily headed the list. This was followed by "murder plays", "The Singing Spider", "Appointment with Fear", and "I Killed the Count" (referred to by one boy as "I Killed the Cow"). Miss Scott rightly concludes that it is idle to blame one particular programme, such as Dick Barton, for children's fears and nightmares, particularly when it figures in a programme definitely designed for their ears, and is placed at a time (so we are told) "which will not interfere with pupils' homework". Most of the boys saw, heard, or read, week after week, numerous violent and hair-raising episodes, and Dick Barton was plainly just one of a far larger group of exciting influences that played upon their youthful imaginations. But such a conclusion could hardly have been reached—far less substantiated—with-

out the numerical evidence which was thus systematically collected.

A more important and persistent set of problems, which can be dealt with in much the same way, are those relating to the causes and treatment of such conditions as backwardness, social maladjustment, juvenile delinquency, and the like. Since the war there has been much speculation about the conditions responsible for crime among children of school age. Is it due to poverty, to lack of intelligence, to psychoneurosis, to weak home discipline, to removal from parental care at an early age, or to war-time incidents such as raid shock and evacuation? Here, by taking a control group, and then counting up and comparing the frequencies of the suspected factors first among the delinquent cases and then among the normal law-abiding youngsters, clear and objective evidence can be procured.

THE NATURE OF MENTAL MEASUREMENT

In the inquiries we have mentioned so far, no psychological variables have been actually subjected to measurement. But, naturally enough, more precise and more detailed information could be gained if, instead of just counting frequencies, we went further, and succeeded in assessing the strength of the various reactions or the degree of individual differences. However, it is just at this point that we are warned off by the prohibitions of those critics, who categorically assure us that "whatever else they may be, conscious phenomena, unlike material phenomena, are not measurable".

In considering the truth of these sweeping denials, let us begin by noting that they assume that everything in the universe can be divided into two categories—those that can be measured and those that cannot. In contrast to this view, we venture to contend that measurability is a *relative* property. Since this point is fundamental, and yet so constantly overlooked, it calls for a few words of explanation.

The truth of the statement follows immediately from the postulates laid down by mathematicians and logicians, like Russell or Huntington, or physicists like Campbell and Jeffreys. Measurement may be defined as the assignment of numerals to objects (perceptual or conceptual) according to a working rule. Whether or not, in the case of some particular type of objects, such an assignment is of any scientific value depends on the degree to which the relations between the objects correspond with the relations between numbers. For example, the assignment of numerals by the post office to the houses in a street or to the subscribers to its telephones has little more than a designatory value: if, in its wisdom, the post office assigns those numerals according to some kind of intelligible rule, then, by the terms of our definition, those numerals will constitute a kind of measurement, though admittedly it will be measurement of a rather crude and uninformative sort.

Now, in their discussions of measurement, most critics of the psychologist's procedures think solely of a simple linear scale, with the numbers expressed by the familiar Arabic notation, and amenable to the elementary operations of arithmetic. That, however, is a very restricted notion. The decimal scale is just a human artefact, the corollary to the possession of two hands with five fingers on each. Not all civilizations have adopted it. No doubt it has its advantages; they can be readily observed, if one tries to work sums in the Roman notation, like multiplying MDCCCXLIII by XCIV. A four-footed creature with three toes, such as the primitive horse, would no doubt have preferred the British duodecimal coinage to the American or the French, and British long measure to the metric system; but he would probably have insisted on having twelve shillings in the pound as well as twelve pence in the shilling. Evidently, therefore, the customary kind of measuring scale has no fixed foundation in nature: its adoption is just an affair of convenience or convention.

Now nearly all modern computing machines, instead of adopting a base or "radix" of 10, 12, 3, or 4, use two numbers

only—0 and 1. Thus, instead of writing MDCCCXLIII or 1843, they would write 11100110011, that is $1 \times 2^{10} + 1 \times 2^9 + \ldots + 0 \times 2^2 + 1 \times 2^1 + 1 \times 2^0$. It will be seen at once that this greatly extends our notion of measurement: things now become measurable that did not seem measurable before. For instance, since 1 can denote the presence of an attribute and 0 its absence, this new notation can treat any form of qualitative classification as a simple kind of measurement. Now turn to a difficulty of the opposite type. Here it is necessary to remind our critics that, in modern mathematics, complex multidimensional attributes can quite readily be expressed in metrical form by adopting what is known as "matrix" notation—a notation regularly exploited by factor analysis. If the term sounds somewhat formidable, the principle is quite clear. The Hindu or Arabic notation uses "place" in a one-dimensional *row*: 1843 means 1 times 10^3 *plus* 8 times 10^2 *plus* 4 times 10^1 *plus* 3 times 10^0; matrix notation uses places in *both* rows *and* columns simultaneously; and can thus represent patterns or schemes as well as linear or scalar qualities. Such devices enable us quite easily to deal with configurations—with *Gestalten*, to borrow a favourite catchword of our critics.

Between the two extremes we can now draw up, as it were, a scale of scales, ranging from types of measurement which would ordinarily be considered scarcely worthy of the name to the most complicated forms of all. A list of the main types is given below, with a few illustrative instances, the last example in each set being drawn from the field of mental measurement.

Scale	*Operation*	*Examples*
1. Designatory.	Mere identification.	Numbering of houses. Code-numbers of cases for reference.
2. Classificatory.	Determination of equivalence of objects.	0 or 1 to denote presence or absence of properties. Harvard method of cataloguing. Discrimination of sexes.

Scale	*Operation*	*Examples*
3. Ordinal.	Determination of greater or less.	Relative hardness of minerals. Relative pleasantness of colours. Order of merit in class.
4. Interval.	Determination of equivalence of differences.	Time. Fahrenheit Scale of Temperature. Energy. Intelligence quotient.
5. Ratio.	Determination of equivalence of ratios.	Length, Weight, Resistance, Kelvin's Absolute Scale of Temperature. Thurstone's Absolute Scales.
6. Multi-dimensional.	Determination of complex numbers.	Matrices or tables of numbers. Mark sheet for N boys in n tests.

Each scale is derived from the preceding by introducing one further postulate. Consequently, the operations mentioned as characteristic of the scales are cumulative. Hence any one of the scales described involves not only its own specific operation, but also all the preceding; for example, measurement by a "ratio scale" (line 5) implies equivalence of differences (line 4), a determinate order (line 3), repeated classification and subclassification by dichotomy (line 2), and identification of the persons measured (line 1).

Now the criticisms urged by Chambers, Zangwill, and the older "intuitionists", as well as the replies offered by the "psychometrists", seem tacitly to assume that measurement in psychology must always mean measurement on a scale of type 4 or 5. They forget that binary classification is itself a form of measurement; they ignore the common use of ranking or order of merit, which has been recently restored to statistical respectability by the work of Kendall; and they entirely overlook the application of matrix measurement to complex configurations. Above all, their assumption that all properties can be divided, by some absolute line of demarcation, into two distinct classes—measurable

properties (which they suppose are always physical) and non-measurable properties (which they assume include all the psychological)—rests on a entirely untenable simplification of the facts.

So far, then, as our first problem is concerned—the theoretical problem of validity—we seem forced to admit that, in the field of educational psychology, counting, measurement, and the various statistical procedures dependent on them, are at least theoretically possible, and that, provided the appropriate conditions are observed, we may justifiably employ some kind of metrical procedure, if we really want to. We may now therefore pass to the far more important question—that of the practical value of such methods. Probably the most convincing way to deal with this second issue will be to enumerate some of the more important results which have been achieved by statistical research and which certainly could not have been reached without it.

THE PRACTICAL VALUE OF STATISTICAL RESEARCH

The problems that confront the practical teacher are of two main kinds—problems of training and problems of individual differences. First of all, he wants to discover what are the best and most effective methods of training his pupils. On the intellectual side, for instance, he would like to know which is the best way of teaching this subject or that, and whether he should alter his material or mode of presentation according to the age, sex, or social background of the children; on the side of character he would like to know what degree of self-control or discipline he can expect at each succeeding year of life, what are the broad principles of moral education and mental hygiene, and so on. These are quite general questions, relating to the mind of the child as such. But secondly it is equally essential for the teacher to have a fairly clear idea of the ways in which one particular child may differ from another, to know, for instance, how far his teaching must take account of inborn differences in general

intelligence, in special aptitude, or in temperamental constitution, and, if so, to what extent such individual differences are likely to limit the success he may hope to achieve with this type of child or that.

INDIVIDUAL DIFFERENCES

Strangely enough, although it was the first group of problems that mainly attracted the educational psychologist in the early days, it is the second group—the problems of individual differences—that have proved by far the most amenable to research; and it is in this particular field that the greatest discoveries have been made by psychology during the past fifty years.

Of these, the earliest and the most important were the general methods of assessing intellectual abilities and attainments. By far the best-known device is the construction and scaling of standardized tests. However, before we can apply our techniques of assessment, we need first to determine what are the fundamental qualities which we may legitimately seek to measure. For centuries psychologists have disputed about what are the essential faculties of the mind: some fifty years ago the majority had apparently decided, with Mill, Ward, and Spearman, that no such faculties existed—only some single general process, like association, discrimination, or possibly "mental plasticity"; they could not agree *which* was the most fundamental, though they were all convinced that only *one* was fundamental. But their conclusions remained rather unconvincing, because they rested almost entirely on armchair speculation, with no systematic attempt at objective verification. At the beginning of the century, however, a special technique, called, not perhaps very aptly, "factor analysis", enabled the investigator to determine, by purely empirical methods, what are the fundamental factors of the mind. The application of such methods has taught us that each individual personality is organized into a hierarchy of abilities or tendencies. It has led us first of all to distinguish

between cognitive or intellectual characteristics, on the one hand, and orectic (that is, conative and affective) characteristics on the other. And on each of these two main aspects it has revealed a number of narrower classificatory factors which psychologists usually identify with concrete "cognitive abilities" and with definite "temperamental traits". Among the cognitive factors unquestionably the most significant, at least for the educationist, is what is popularly known as "general intelligence". Fortunately the combination of statistical and experimental procedures has furnished us with excellent methods of measuring it. But of almost equal importance is the discovery of what are usually termed "special aptitudes"—such as memory, and verbal, numerical and practical ability. It is our knowledge of the way in which these special aptitudes emerge and mature that helps, or should help the educationist, in the perplexing problem of allocating boys and girls to suitable types of secondary school at the age of eleven plus, and again, at a later stage, in giving them sound vocational advice.

To demonstrate the existence of these special abilities, and to determine how they are related to each other, would have been quite impossible without the aid of a rigorous statistical technique like factor analysis. May we emphasize, however that it is quite wrong to suppose, as both critics and supporters of factorial methods so often do, that factorial methods alone are sufficient to establish concrete conclusions? Their function is not to make novel discoveries, but merely to provide a more cogent logical procedure for confirming or confuting rival hypotheses. The hypotheses themselves have nearly always to be suggested by non-statistical work. As Udny Yule has expressed it, "the chief value of statistics is to provide a rigorous verification for conclusions tentatively reached on vaguer and less objective grounds". And in psychology it is the combination of statistical techniques with experimental techniques, or with case-study methods, that is the surest key to progress.

A third contribution of the greatest practical importance has

been the determination of the way in which intellectual abilities are allotted by nature. Perhaps the most surprising result of the early surveys, carried out in London and elsewhere, was the wide range of individual differences. Teachers whose experience has been restricted to just one or two schools or one or two districts know well enough that their pupils differ by inborn constitution, but they seldom realize how widely such differences may range, when the whole population is under review. Take 1,000 children aged ten: the dullest will have a mental age of five or even less, the brightest a mental age of at least fifteen. And over this broad stretch how is intelligence distributed? Is it distributed like property or income, where the majority have comparatively little, and those who have much are relatively few? Or is it distributed like height and other biological characteristics, where the average type is the commonest, and the dwarf as rare as the giant? Statistical surveys leave no shadow of doubt that the latter alternative is correct. What is more, they show that the distribution follows, to a close approximation, the well-known curve of error or chance. Abilities are scattered like shots from a gun; and, as every gunner knows, when a sufficient number are fired, the proportion falling so much to the right or so much to the left of the target can be predicted with considerable precision. It follows that the psychologist can calculate, with a high degree of accuracy, how many pupils in a given area will fall above or below this or that particular borderline, how many schools for the educationally subnormal the local authority should aim to build, and how many scholarships or special places it should provide in its grammar schools, its trade schools, its special schools, and the like.

The discovery that about 1½ per cent of the population are so defective that they cannot be taught in ordinary schools at all, and that another 10 per cent—the dull and backward—need special instruction in small and special classes, all this has greatly altered our conceptions of education, since the old days of "payment by results". We know now that in such cases neither the

teacher nor the child is to blame for the slow progress achieved; the maximum is irrevocably fixed by the inborn limitations of each child's mental endowment.

Where then should we draw the line between the normal and the definitely subnormal—the mentally defective, as they used to be called? Here the employment of a less familiar device—the "discriminant function" as it is now termed—has led to the introduction of a uniform standard, instead of the widely varying borderline adopted by school doctors forty years ago. The well-known borderline of a 70 per cent intelligence quotient was derived in this way.

Perhaps a still more surprising discovery is the fact that mental growth, like physical growth, slows down rapidly after the onset of puberty, and, with the average person, appears almost to cease by the age of about fourteen. This means that the innate intelligence of the average adult is little, if at all, above that of the average fourteen-year-old child, and, what is too often forgotten, that half the adult population fall below this average level. This fact, of course, is of great importance, not only to educationists, but to all who are interested in the problems of democracy. It accounts for many difficulties which few politicians or sociologists have as yet satisfactorily faced.

The alleged psychological differences between races, sexes, and social classes have formed topics for popular dogmatization even more frequently than the differences between individuals. For example, fifty years ago men were supposed to be innately different from women, and boys from girls; and the differences were declared to be even greater in mental characteristics than in physical. The application of standardized tests and the use of appropriate statistical procedures have shown that the traditional views were very wide of the mark. They have certainly revealed the existence of certain small intellectual differences; but these are smaller on the higher levels (e.g., in intelligence or reasoning than on the lower (e.g., in memory or skin-discrimination); and they are far smaller in affective or emotional traits. Again, the

average intelligence of children from the higher professional classes is 120 I.Q.; that of children of unskilled labourers only 92 I.Q. At first sight this may seem to justify the fact that during the ten years before the war, among those who entered the Universities, nearly three-fifths came from the fee-paying classes and only two-fifths from the elementary school classes. But we must not forget that the latter section of the population is nearly fifteen times as numerous as the former. Hence, if innate intelligence alone were the criterion, there should be between two and three times as many University students drawn from the latter as from the former. But the really important result is this: the differences between the averages of different groups—whether the groups are sexes, races, or social classes—are comparatively small; they are almost completely swamped by the individual differences within each group.

On the temperamental side, it must be admitted, our classification of mental characteristics, and our attempts at accurate measurement and efficient testing, have not been so successful as they have proved to be on the intellectual. But even here some degree of progress has been achieved: statistical methods have been applied on a very large scale, first to the study of juvenile delinquents and more recently to the study of young neurotics; and much light has thus been thrown on the causes at work and on the efficacy of various types of treatment.

PROBLEMS OF TRAINING

When we turn to the second group of problems the answers are far less clear. A good deal has been accomplished in the study of the learning process and of the merits of different teaching methods, especially as regards the more elementary subjects. But many of the disappointments in this field of research seem due to the fact that those who have hitherto investigated such problems have so often failed to adopt an effective statistical technique. In studying individual differences, correlational

analysis has been invaluable, though it is not (as many critics apparently assume) the only useful tool. In studying teaching methods, on the other hand, what is called the analysis of variance seems to offer a more suitable procedure. But, although in other branches of biological science this has freely been applied, in the human field it has, until quite recently, seemed almost an unknown device.

If, then, we may now sum up this brief review of practical results, we may perhaps put the general outcome as follows. With the aid of statistics, much has already been done in the field of individual differences, and much might be done in the field of mental training, that could not possibly be accomplished without it. And so we reach our third main problem: the improvement and further adaptation of statistical methods for use in educational psychology.

FUTURE DEVELOPMENTS

As we have already seen, the commonest, and in many ways the most serious, of the criticisms which the statistical psychologist has to face, springs from the special nature of psychological data. Mental processes, we are constantly reminded, consist, not of isolated and separately varying quantities, but of configurations or patterns; and, admittedly, most quantitative techniques have been developed with simple unidimensional variables chiefly in view. But it so happens that, during the last few years, for both scientific and practical purposes, there has been a rapid development in mathematical devices for dealing with configurations and patterns. We have already mentioned the introduction of matrix algebra, with which should go the theory of "groups" and "sets". The work of the French has carried these theories still further. The most recent developments have been largely a by-product of war-time problems. Perhaps the most striking applications have arisen in connexion with those particular patterns which are transmitted by ordinary and by wireless

telephony. This is the type of problem with which Wiener, at the Massachusetts Institute of Technology, and Shannon, at the Bell Telephone Laboratory, have been actively concerned. We venture to suggest that mathematical procedures which they have worked out may supply precisely those statistical models which the psychologists requires in his attempts to apply quantitative exactitude to still more complex fields.

To describe the general theory, and show how it may help to meet many of the difficulties our critics raise, would be too long and too technical a matter to embark on here. But we may briefly mention one or two advantages. First, the whole approach is based, not so much on deductions from theoretical postulates, as on inferences from practical requirements. Secondly, the collection and communication of information is treated as a case, not of simultaneous and instantaneous "projection" (as is done in the theory of the so-called "projection" tests), but as a problem in which time and causal agencies, both controlled and uncontrollable, play an essential part. Thus the common criticism that "statistical estimations of the psychologist give a static rather than a dynamic representation" is to a large extent forestalled.

In particular, this mode of approach makes it far easier to introduce what are known as "sequential" procedures. In educational research one of the commonest sources of disappointment arises from the continual discovery that a conclusion reached in one particular investigation is apt to break down in the next. A research student, for example, spends twelve months constructing a small battery of tests for selecting children for grammar schools or technical institutes; he finds that his tests if properly weighted, will give a multiple correlation of (say) 0·8 with the estimates of a good teacher or with the subsequent school performances of his sample of pupils. A few years later, he or his successor tries the same set of tests with a second sample. Every experienced investigator knows only too well what will happen. If we use the same set of weights, the multiple correla-

tion will be far lower; and quite conceivably a test that previously furnished quite a promising result now seems worthless. Prolonged and progressive inquiries, using all the devices of revalidation and cross-validation, are therefore indispensable. Here lies the practical advantage of appointing an educational psychologist as a permanent member of the education officer's staff. Indeed, the value of the schemes of mental and scholastic tests, developed and published by the London County Council between the two wars, derived largely from the fact that it was possible for the Council's psychologist, holding as he did an internal and permanent post, to collaborate with teachers and others on an authoritative basis, and to continue research on each important test-procedure until it seemed to satisfy all reasonable and practicable needs. In this way official investigations can be planned with statistical requirements duly considered from the very outset, instead of the statistics being applied as a kind of afterthought to data collected with no prior regard to logical cogency.

Our final conclusion, therefore, is this. First, the objections commonly levelled against the various statistical procedures used by educational psychologists are based on too narrow a view of what such procedures imply, and what they can legitimately do. Consequently, none of the criticisms really affect their general validity. Secondly, such procedures have already solved a number of urgent practical questions which could scarcely have been dealt with in any other way. Thirdly, though there are undoubted defects both in the theories and the practice of statistical psychologists, recent developments should do much to correct them. Thus, what is really needed is not less statistics, but more. Statistics must cease to be an isolated technique. Many of the weaknesses to which the critics point can readily be overcome by a closer co-operation between statistical and non-statistical investigators. The statistician must himself keep abreast of the newest procedures; but at the same time those who collaborate with him, and supply him with his data, must learn to appreciate the logical basis of his work.

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XIV

STUDIES OF ANIMAL LEARNING

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THE USE OF ANIMALS IN EXPERIMENTAL PSYCHOLOGY

THE study of animal learning is a subject which has been much neglected by psychologists in this country. The knowledge we have of this subject is due, with some notable exceptions, to the work of American psychologists. Since the relatively pioneer work of Lloyd Morgan in this country and Pavlov in Russia the bulk of both experimental and theoretical work has come from America and the British work has fitted into the framework of the American theorizing.

The psychologist rarely uses animals in a truly comparative way. Instead, he uses them as research tools in the investigation of problems of general psychological interest. The animals are used primarily as an attempt to simplify the experimental conditions in learning situations. An outstanding difficulty in psychological research is the impossibility of achieving any measure of complete control over the variables influencing behaviour. The external stimulating conditions can, largely, be controlled but the internal environment of the subject cannot; this is especially noticeable in human subjects who differ from each other markedly in general attitudes, co-operativeness,

interest, anxiety, etc., any of which may affect performance at a particular task and may be extraneous to the variables under investigation. The psychologist uses animals, therefore, partly on the assumption that, by doing so, he is reducing the number of such extraneous and uncontrollable variables, and partly because he can manipulate more easily the environment in which the animal lives, and the motivation for which it learns in a manner which, with human subjects, would be regarded as, at the very least, socially undesirable. One effect of the experimental psychologist's attitude of regarding animals as something like an experimental convenience has been to restrict attention to a very few species, which have proved themselves suitable for learning experiments. As Beach has pointed out, nearly 70 per cent of all papers published by psychologists during the past decade, where animals have been used at all, have used the Norway rat, while other mammals accounted for just under 30 per cent. The tendency has arisen in fact to use the Norway rat if one is interested in "Trial and Error Learning" or the chimpanzee if one wishes to study "insight". It may well be that a broadening of the area of study to other species would be most beneficial.

THE NATURE OF LEARNING

In learning to run a maze or to open the door of a "puzzle box", the rat shows a progressive modification in his behaviour from trial to trial. He gradually gets quicker and he gradually reduces the number of mistakes made. This gradual and progressive change in behaviour is regarded as the prototype of all learning by the so-called "Behaviourists". In Thorndike's terms, an animal placed in a new problem situation will, provided it is adequately motivated, respond to it by running through its repertoire of possible actions in a random manner. The cat placed in a puzzle box will try to squeeze through the bars, will scratch the floor, wash itself and so on, and eventually

will depress the lever which will unlatch the door and release it. On subsequent trials, this "correct" response will appear earlier and earlier in the sequence, but will for a considerable number of trials be preceded by some of these "incorrect" responses. Eventually the required response will be produced as soon as the problem is presented. The problem is then said to be learned.

It has been recognized for many years, both in popular practice, and in psychological theory, that learning and incentives are very intimately connected. Indeed it is a common, though not universal, modern psychological practice to regard learning as a progressive modification of behaviour which is adaptive in allowing satisfaction of the organism's needs in a manner which is more adequate and usually more economical in energy expenditure, than was previously possible. This satisfaction of needs is, as Humphrey has pointed out, so central to the concept of learning, that modifications of behaviour which preclude it are not normally regarded as learning, but are called "stereotypes", "fixations" and so on. Clearly this question to a great extent depends on the verbal definitions used.

This differentation cannot, however, be pushed too far. Many cases are cited in the literature where such satisfaction has been withheld on many trials without marked diminution in the rate of behaviour modification. In fact Skinner has shown, using an apparatus in which the rat is required to depress a lever to obtain food, that the rate of responding may increase when food is withheld on some trials. He found that when a fixed ratio of unrewarded to rewarded trials is used, the frequency of response becomes higher as the frequency of reinforcement becomes lower. When one trial in 192 was reinforced, a significantly faster rate of response was obtained than when the ratio was one to ninety-six or one to forty-eight. Conversely, rewarding at a fixed time interval, once in three minutes, as compared with once in nine minutes, for example, gives a slower rate of response the greater the time interval between reinforcements.

This phenomenon, however, does not necessarily contradict the general assumption that reinforcement is a necessary condition for the demonstration of any learned response. It has been shown that stimuli which have been associated with food can themselves acquire a "goal significance". Grindley placed young chicks in a straight runway at the other end of which were grains of rice under glass, so that the chicks could see but not eat them. He found that running speed increased during the first four or five trials, indicating that learning was taking place, but that speed subsequently fell off (Fig. 8). Wolfe trained chimpanzees to "cash" tokens like poker chips in a slot machine, in exchange for grapes. Red chips were worth two grapes, blue one, and brass none. He found that, given a heterogeneous collection of chips, the animals would pick out all the red first, then the blue, but would ignore the brass ones. Furthermore, the chips would then be used as incentives in learning experiments. Complex visual or spatial discriminations could be learned for a "reward" of a red or blue chip, but not for a brass one. More recently, Danziger, working at Oxford, has been able to show that stimuli associated with a learned task can also act as a drive. Rats who had learned to run for food in a straight runway, still ran significantly faster when the hunger drive was satiated than satiated rats who had never been in the runway when hungry. These, and other similar experiments, show not only that such stimuli can and do act as secondary reinforcements, but also, that they derive their function from their association with primary reinforcements, that is, with food, water and so on. As in Grindley's experiment, secondary reinforcements cease to be effective unless themselves reinforced by the primary ones from time to time.

Primary reinforcements, therefore, appear to be of fundamental importance in determining whether or not learning will occur. Their precise function in learning is, however, a matter of

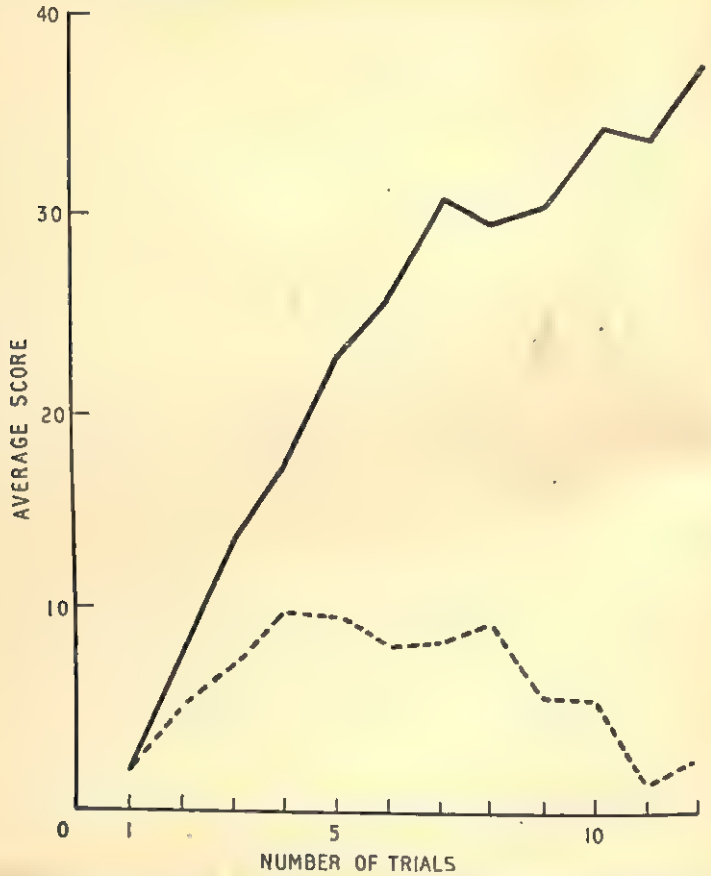

FIG. 8.—From Grindley, G. C. "Experiments on the Influence of the amount of reward on learning in young chickens". *Brit. J. Psychol.*, 1929, XX, 179 *(published by kind permission of the author and editor)*

considerable controversy, between those who regard the "goal-reaction" as the chief determinant of learning, and those who regard it as a necessary condition for the *performance* of a learned activity, but as unnecessary for its acquisition. Pre-eminent amongst psychologists of the first group are Thorndike and Hull, while Tolman is the main protagonist of the second.

THORNDIKE'S AND HULL'S LEARNING THEORIES

Thorndike's views are precisely stated in his well-known Law of Effect. In its earlier form this law stated that, other things being equal, actions which are accompanied or closely followed by satisfaction to the animal will tend to be repeated, while those followed by discomfort will tend not to be repeated. In other words, the effect produced by a response, in terms of "reward" or "punishment", will strengthen or weaken the associative bonds between the stimuli and the response. To avoid the anthropomorphism in these terms, experimenters have, in general, interpreted "satisfactions" or "rewards" as food for a hungry rat, water for a thirsty one, etc., and "discomforts" or "punishments" as electric shock, confinement in an enclosed space, or sometimes, simply the withholding of "reward".

A very great deal of experimental work has been stimulated by the formulation of this law. This has led to its reformulation in terms that, while rewards do act in strengthening connexions between stimuli and responses, punishments have very little effect in weakening them. Experiments have, in fact, shown that the effect of punishment is relatively unpredictable, and is, at present, very difficult to formulate. Typically, punishment is used in learning experiments by administering a mild electric shock whenever an incorrect response is made, such as choosing the wrong door in a discrimination experiment, or entering a blind alley in a maze. The usual effect of administering electric shock for a wrong response, to a hungry rat, receiving food for the correct one, is to produce quicker learning than with food alone. While this appears to support the original form of the law of Effect, Muenzinger and various collaborators, as well as Drew, have shown that similar results can be obtained if shock is given when the correct response is made. Drew's results, which may be used as an example, are shown in Fig. 9. Four groups of rats, were trained on a visual discrimination problem. All were hungry and received food when the correct response was made

but three of the groups received an electric shock in addition, one on touching the incorrect door, the second on touching the correct door, and the third after the correct response was made when eating the food. The results corroborate Muenzinger's view that punishment anywhere after the point of choice is

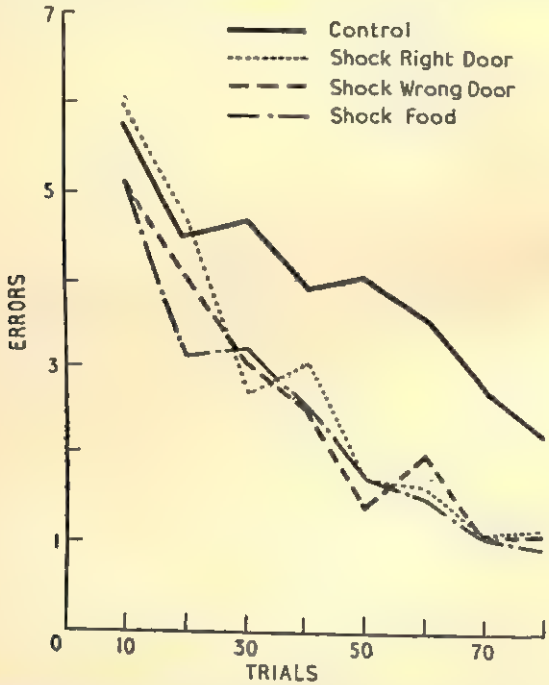

FIG. 9.—From Drew, G. C. "The function of punishment in learning". *J. Genet. Psychol.*, 1938, lii, 262

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equally effective in facilitating learning. Examples have also been recorded of rats showing rapid and lasting fixations on the response for which they have received a fairly strong shock, even though the response was not rewarded.

On the other hand, it is generally agreed that no learning is apparent unless the response is reinforced in some way. Tolman

and his collaborators have shown that the rate of learning varies with the degree of hunger; that hungry rats which receive no food in a maze show little learning compared with those who do; and that the removal of the reward when the task is partly learned produces a rapid increase in both time and error scores. (Fig. 10.) It can be shown also that a preferred food will produce quicker learning than a less preferred food.

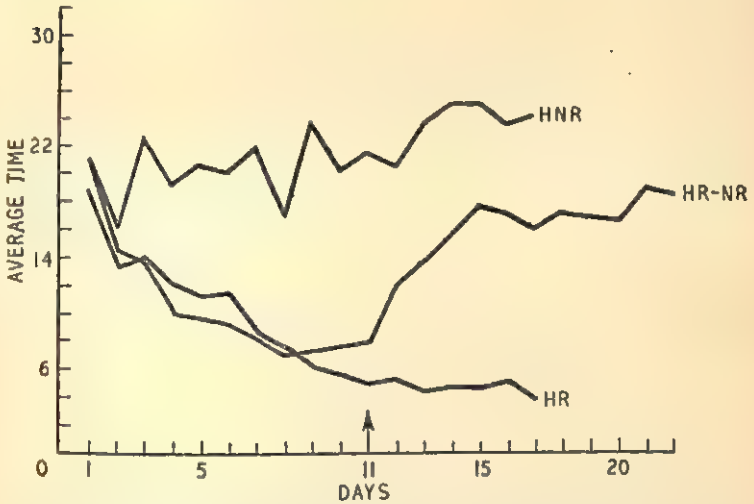

FIG. 10.—From Tolman, E. C. and Honzik, C. H. "Introduction and removal of reward, and maze performances in rats". *Univ. Calif. Pub. Psychol.*, 1930, iv, 263 (*published by kind permission of the University of California Press*)

In an attempt to explain the mechanism by which the reinforcement determined learning, Clark L. Hull developed a theoretical system of postulates, and deductions from them, which relied to a great extent on the principles of the classical conditioned reflex. In doing so, Hull advanced considerably from the crudities of Watsonian Behaviourism. He does not postulate series of chained reflexes, but attempts to apply the principles of the conditioned reflex to behaviour patterns, using whatever sequences of responses as units which prove convenient. His

method is to define his postulates as rigidly as possible, deduce from them what behaviour should occur in given situations, and submit his deductions to experimental verification. His system is probably the closest approximation that psychology has yet made to a scientific theory aiming at predictability as the criterion of its validity.

Combining the known effects of a reward in producing learning with the principles of generalization and of temporal gradients in conditioned reflexes, Hull postulated what he called the "Goal Gradient Hypothesis". This states that, other things being equal, reactions will be conditioned the more slowly, the more remote in time or space they are from the goal reaction. From this he was able to deduce, for example, that errors nearer the goal would be eliminated more rapidly than those nearer the start and that blind alleys in a maze which pointed in the direction of the goal would be more difficult to eradicate than those not so pointing. Both of these have been confirmed by experiment. Hull also deduced that the speed of the rat would increase as it neared the goal. Testing this experimentally, he found that the speed did increase, and that, apart from a terminal retardation, the increase in successive measured segments followed approximately a logarithmic law. Drew found that this gradient in running speed appeared to be due to factors other than the "pull" of the goal. It was present only when several trials were given in close succession. Analysis of the individual trials showed a steeper gradient on each successive trial, due, apparently, to an increasingly slow start rather than to an increasingly rapid finish (Fig. 11). Satiated rats, and rats fed in each timed section of the runway also showed the gradient. He argued from this that the speed of locomotion gradient could not easily be used to support the goal gradient hypothesis.

Though Hull's theory has in recent years been greatly refined and considerably complicated, it still relies on a modified Law of Effect as a cornerstone. It is stimulus-response connexions which are reinforced and the law of effect is made to apply to "per-

formance" in the latest Hull revision, which clearly distinguishes between performance and learning. One of the greatest difficulties the theory has to face is the so-called "latent learning" demonstrated by Tolman. Tolman found that if hungry rats are placed in a maze they showed little or no evidence of learning even after many trials. If, however, these same rats are then rewarded in the

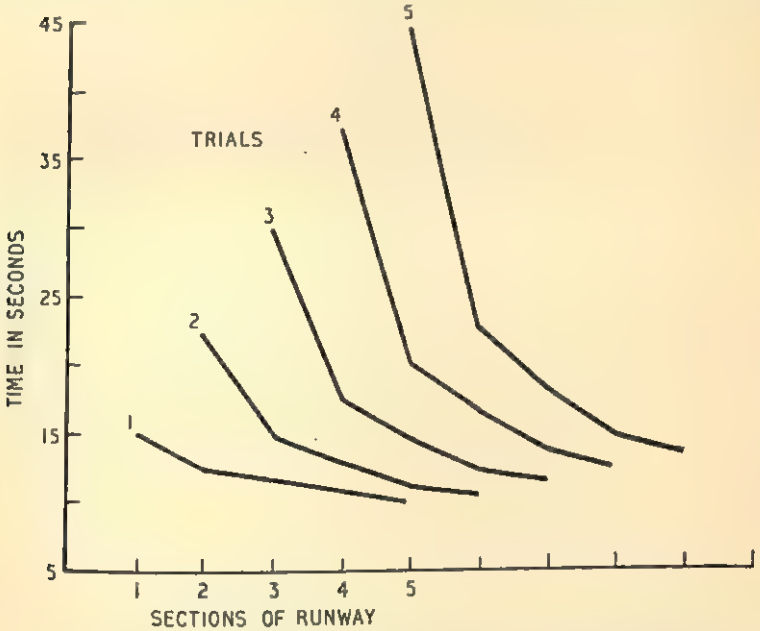

FIG. 11.—From Drew, G. C. "The Speed of locomotion gradient and its relation to the goal gradient". *J. Comp. Psychol.*, 1939, xxvii, 359
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maze by being given food when they reach the food-box, they show a rate of learning considerably in excess of that of control groups fed in the maze from the start (Fig. 12). From this, he deduces that the rats had been learning the maze during the no-reward period. The function of the reward was to produce overt signs of learning, not to control the learning itself.

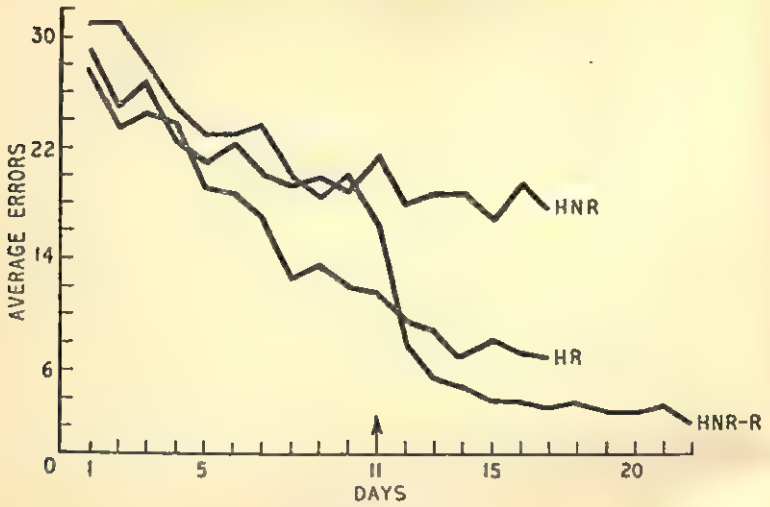

FIG. 12.—From Tolman, E. C., and Honzik, C. H. "Introduction and Removal of reward, and maze performance in rats". *Univ. Calif. Pub. Psychol.*, 1930, iv, 267
(published by kind permission of the University of California Press)

OPPOSED VIEWS OF LEARNING

Tolman's whole approach is quite different from Hull's. He does not believe that learning can be explained by any system, however complex, which postulates the sorting out of initially random responses by some reinforcement mechanism. For him, the animal approaches any problem in an active, organizing manner. Responses are seldom or never random. Krechevsky required rats to obtain food by making a complex visual discrimination. This was not an easy problem for a rat, and the learning curve obtained showed a relatively long period before any marked improvement was apparent. This long "pre-solution" period enabled Krechevsky to analyse the responses made to see whether or not they could be called random. He found on the contrary, statistically reliable sequences of responses. The rats would go first to the left-hand side, for example. After a while, this response would drop out and another, for example,

a regular L.R. alternation would take its place. He even found double alternation sequences, such as L.L.R.; L.L.R.; L.L.R.; before these spatial "hypotheses" as he called them, gave way to visual discrimination. Thereafter, learning was rapid.

For Tolman, learning consists in forming, modifying, and reflecting these hypotheses, or "cognitive maps" as he calls them. Motivation produces, by an "expectancy" principle, overt performance of the activity, but does not control the learning. Tolman's theory is far more flexible than Hull's but is much less precise, and clearly defined.

Unfortunately, there is no sign of a crucial experiment which will decide between these two main systems. Stimulus-response theorists explain away latent learning in terms of uncontrolled secondary reinforcements, while Tolman's adherents strive to eliminate all such factors. It is certain that a great deal more experimental work is required before any precise knowledge of the relationship of the goal-reaction to the response preceding is gained. It is to be hoped that British psychologists will play a rather bigger part in the future than they have done in the past in the field of animal behaviour. Perhaps it would not be out of place here to plead for more psychologists with some zoological training to enable us to see a little further than the rat.

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XV

THE USE OF INTELLIGENCE TESTS IN SOCIAL SURVEYS

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THE CHARACTERISTICS OF SOCIAL SURVEYS

In the first place, a survey is fact-finding. In the second place, a survey is comprehensive and inclusive; it gathers the facts about a complete population. It is not the true function of a survey to test a hypothesis or to pose a problem, though the survey may provide in practice the facts which enable the problem to be solved or the hypothesis confirmed. To find, for example, whether children with high I.Q.s excelled both in academic and practical school subjects, a survey would not be necessary. A more limited field of inquiry could establish the situation beyond reasonable doubt, without a comprehensive survey of the school population. The term population does not necessarily denote every man, woman and child in the country, but it does imply that any results should be applicable to the survey group as a whole. If, for example, we make a survey of eleven-year-old Scottish children, every effort must be made to include all such children, and not just some of them. And should it prove impracticable to survey the whole population, any sample selected must be sufficiently large and unbiased for it to be regarded as truly representing the whole population.

There are several kinds of survey that may properly be termed social. The most venerable and efficient are the financial or

economic surveys. The Domesday Book is the record of the earliest in this country, and there is still conducted an annual survey by Her Majesty's Commissioners of Inland Revenue. It is both fact-finding and comprehensive. There are periodic demographic surveys in the form of the Census, as well as more recent and specialized surveys in connexion with the Royal Commission on Population. Of the more specifically sociological surveys, we may mention the "Statistical Accounts" of Scotland, the third of which is now in progress, Booth's London Survey, Rowntree's York Survey, and such more recent surveys as that of the Merseyside. There are a number of psychological surveys, the grandfather of these perhaps being Galton's various inquiries. Others have followed, such as the Mental Deficiency Surveys of Dr. Wood and Dr. Lewis, the Bath Survey of Dr. Fraser Roberts and his colleagues, and the two Scottish Mental Surveys of 1932 and 1947.

The value of such surveys does not lie in the collection of facts as such but in the action that follows from a fuller knowledge and understanding of the facts. Sometimes the action is direct. Notice to pay follows completion of Schedules A and D. At other times the survey draws attention to certain conditions, or leads to fuller understanding. The surveys of Booth and Rowntree revealed the extent of poverty and contributed to the improvement of the social conditions of the poor. Demographic surveys help in determining policies of social welfare, immigration or family allowances. Surveys of intelligence are not without influence on educational policy. Social surveys, also, are necessary to ascertain the trends and developments in the structure of the community. Rowntree's York Survey of 1900 was repeated in 1936, Bowley's 1913 survey of the Five Towns was repeated ten years later, and the Scottish Mental Survey of 1932 was repeated in 1947. Both the York and the Five Towns surveys showed clearly the improvement in social conditions over the intervening intervals, and the two Scottish Mental Surveys showed that the decline of average intelligence, which was feared

in certain quarters, did not appear to have occurred. Finally, such surveys serve a useful purpose in the research field by supplying a framework of general reference for more particular investigations.

THE CONTRIBUTIONS OF PSYCHOLOGY TO SOCIAL SURVEY WORK

Of recent years there has been a marked extension of both social services and social research. It has become apparent that we cannot act in one aspect of social conditions, such as education or housing, without causing repercussions in other aspects. The centre of interest is shifting from study of the conditions in which people live to the kind of people who live in these conditions, and it is becomingly increasingly clear that we must concern ourselves not only with the material conditions of living but also with what can be called the psychological quality of the population. Let us consider two examples of this connexion between social and psychological factors. The material conditions of poverty are well known, but the age-old question remains, whether the poor live in poverty because of social incompetence in the form of inadequacy of character or intellect, or whether the social handicaps of their conditions of life are too great to be overcome, and their poverty is not the consequence of their inadequacy, but the cause of it. Data from the 1947 Scottish Mental Survey show that the average level of intelligence of children living in the adverse social conditions is considerably lower than that of children in the favourable conditions. Yet there is also a considerable number of intelligent children living in poor conditions and a somewhat smaller number of dull children living in the better conditions. The truth appears to lie between the two extreme points of view. Another example of the desirability of information about the quality of the population is the redistribution or internal migration of the population. The 1951 Census returns for Scotland show no marked change in the proportions

of the total population living in the four different types of area (Cities, Large Burghs, Small Burghs and Landward Areas) as compared with 1931.[1] The Scottish Mental Survey data show that about 89 per cent of the children are, at the age of eleven, still living in the same kind of area as they were born in. The average intelligence test scores of these children in the four types of area are shown in Table 1, and the average test scores of the migrants in Table 2.

Though the average test scores of children born in the different types of area are not greatly different there appears a distinct tendency for the families of the more intelligent children to migrate to the cities, particularly from the Other Areas, and a tendency for the less intelligent to migrate to the Other Areas. Had this trend been existing over any considerable period of time, we should expect a greater difference in the average intelligence of children in cities and Other Areas than we at present find. It may be that the situation is analogous to that of intelligence and family size, where the lower average intelligence of large families does not appear to involve a lowering of the average intelligence of the population.

These are but two instances of the way in which a survey of intelligence can throw light upon, or add complication to, sociological facts. There remains, however, the question of whether we can in fact measure the intelligence of a population.

THE MEASUREMENT OF INTELLIGENCE

We can define precisely what a man's income is, what his occupation is, how many rooms are in his house, how many persons live in the house, and so on. But we cannot so define his intelligence, or even his children's intelligence. There is no agreed definition of intelligence. Even if we define it in terms of "g", and agree what "g" signifies, we still cannot measure "g" directly. Intelligence tests measure a number of things—general

[1] Preliminary Report on the Fifteenth Census of Scotland, H.M.S.O., 1951.

TABLE 1

AVERAGE INTELLIGENCE TEST SCORE (POSSIBLE SCORE 76) OF ELEVEN-YEAR-OLD CHILDREN ACCORDING TO BIRTHPLACE AND PLACE OF PRESENT RESIDENCE

	Birth Place		*Present Residence*	
	Mean Score	*Number of Children*	*Mean Score*	*Number of Children*
1. City	37·2	2,712	37·4	2,596
2. Large Town	36·2	1,015	36·6	1,007
3. Small Town	36·3	354	37·4	362
4. Other Area	35·7	2,334	35·3	2,450
		6,415		6,415

TABLE 2

AVERAGE INTELLIGENCE TEST SCORE (POSSIBLE 76) OF ELEVEN-YEAR-OLD CHILDREN ACCORDING TO MIGRATION

	Children migrating to:		*Children migrating from:*	
	Mean Score	*Number of Children*	*Mean Score*	*Number of Children*
1. City	41·9	128	37·5	244
2. Large Town	38·8	157	36·7	165
3. Small Town	39·3	84	34·4	76
4. Other Area	35·2	345	39·8	229
		714		714

Note 1. The fact that the net loss of the cities equals the net gain of the other areas (116) and the net loss of the large towns equals the net gain of the small towns (8) is a curious but purely fortuitous coincidence.

Note 2. The Large Towns, Small Towns and Other Areas of the Scottish Mental Survey are not the same as the Large Burghs, Small Burghs and Landward Areas of the Census.

intelligence, special abilities, educational achievement. If we define intelligence empirically, as that which is measured by intelligence tests, we must further specify which test, as all intelligence tests do not cover the same ground.

But such difficulties are academic rather than practical, and like Molière's character who discovered that he had been talking prose all his life, we have been measuring intelligence extensively for the last thirty years, without knowing formally what intelligence is. The Binet I.Q. in the Terman Merrill revision has become the basic currency of intelligence testing and it is accepted as a working measure of intelligence. This test has its peculiarities, it is true. It is rather easy for six year olds, very intelligent children of eleven or twelve tend to obtain rather inflated I.Q.s, the standard deviation of intelligence varies with age, and at most ages boys tend to do better than girls. But these peculiarities are known, and due allowance for them being made, a Binet I.Q. is probably as good an index of intelligence as height and weight are of physical development, or income is of standard of living.

There has also developed a fairly consistent pattern in verbal group tests of intelligence, and these tests all have a fairly strong family resemblance. It does seem, though, that special verbal abilities and educational opportunities determine a group test score to a greater extent than a Binet I.Q. Non-verbal group tests have not yet reached the same degree of uniformity of type as the verbal group test. We have available as generally accepted measures of intelligence either the Binet type of test or the group verbal test, the practice in both being well established.

When we come to conduct a survey involving intelligence we are faced with certain practical difficulties over and above that of finding a true measure of intelligence. Firstly, we are virtually confined to a population of school children. The difficulties involved in testing the intelligence of a large and representative population of adults need no elaboration. Secondly, we are virtually confined to group tests, if the survey is to be at all

extensive. In both the Scottish Mental Surveys it was found that about one thousand children was the number which could be tested individually within a period of about three months, and that despite very full co-operation from educational psychologists and other testers throughout Scotland.

In spite of its advantages of easy administration and extensive use, the group test of intelligence commonly used in school promotion examinations has certain defects when used as a measure of natural intelligence. It depends to a considerable degree on schooling and acquired knowledge. Educational policies change, coaching for such tests is not unknown, and what may be common knowledge to one generation may be unknown to the next. Even over the fifteen years separating the two Scottish Mental Surveys, knowledge of the use of bathbrick has been lost to the younger generation. Fortunately the test question involving bathbrick appeared in the preliminary practice test, and was not counted in the final score; but it might have been. For reasons of secrecy and security, also, the group test has to be given to all children on the same day. The weather report for 4th June, 1947, the date of the survey test, states that "there was a general outbreak of thunderstorms. There was little sunshine and although temperature was low, the weather would be popularly described as 'sultry' 'close' or 'oppressive'. Some large amounts of rain fell during the day". Quite a number of schools did, in fact, report that weather conditions were very unfavourable for doing intelligence tests. On 1st June, 1932, the date of the earlier survey test, the weather report states, "there were light northeasterly winds over the whole of Scotland, and the weather over almost all the country was fair or fine".[1]

A perfect day for doing intelligence tests in!

One is tempted to speculate how much greater the difference between the average test score for 1932 and 1947 might have been had the weather conditions been the same for both surveys.

[1] (Acknowledgements are due to the Scottish Meteorological Office, Air Ministry, for the above reports).

POSSIBLE IMPROVEMENTS IN PROCEDURE

Surveys of national intelligence are elaborate and expensive undertakings, and it is right that every effort should be made to render them as reliable as possible. Two proposals to this end are worth consideration. Professor R. B. Cattell for his Devon and Leicester Survey has constructed a "culture free" group test. Whether it is possible to free any test from content related to the conventions and habits of the community is doubtful, but such a test is a step towards the solution of the bathbrick kind of difficulty. For reasons of secrecy, Professor Cattell's test has not been published, and it is not possible to comment upon it. But the construction of any group test likely to remain valid over a period of years and in changing social circumstances is to be welcomed, and there is a case for a group test specifically devoted to population surveys.

The other proposal is by Dr. J. A. Fraser Roberts.[1] He advocates that a randomly selected sample of children be individually tested by a Binet test. A larger group, including the Binet sample, should be tested by a group test, the main requirement of the group test being that it should correlate highly with the Binet test. For a survey such as the Scottish Mental Survey of some seventy thousand odd children he estimates that one thousand individually tested children, and fifteen thousand children group-tested should be adequate.

For the purpose that Dr. Fraser Roberts has in mind, the comparison of the average intelligence of a population after an interval of years, the proposal has much to commend it. Intelligence is measured in terms of Binet I.Q., which has a wider currency than that derived from any group test. The number of children tested individually is manageable, and the degree to which this sample is representative can be established by comparison of their group scores with those of the sample representing the whole population. This procedure, Dr. Fraser Roberts

[1] *The Trend of Scottish Intelligence.* Chapter 8.

claims, should be able to detect quite small changes in average I.Q. at intervals of time.

It is not certain, however, that such a procedure would fulfil all the requirements of a survey in which sociological aspects were as important as the measurement of intelligence. Though a thousand or so children can be shown to be representative of a population in intelligence, or their bias ascertained, it is risky to assume that they would be equally representative in sociological characters as well. For each sociological item recorded there would be required the same check against the larger group. In effect, the operative comparison between intelligence and social conditions would be in terms of the group test scores of the larger group of children. This is not a serious disadvantage. If the group test scores correlated highly with Binet I.Q., they could be converted to Binet I.Q. with small error. In a large group, also, more detailed analysis is less likely to be impeded by smallness of numbers in certain of the categories.

As a result of the experience gained in the Scottish Mental Survey, and after consideration of various proposals and practices in connexion with it and similar surveys, we would put forward the following recommendations for consideration by planners of future surveys involving comparisons of intelligence and social conditions over a period of time. It would appear that individual testing of a random sample of about 1·5 per cent of the population would establish sufficiently the average level of intelligence; provided the 1·5 per cent sample is calibrated by a common group test with a larger 20 per cent sample of the whole population. For the purposes of comparison over a period of years, it is essential that the same individual test be used on each occasion a comparison is desired. Unfortunately the Stanford Binet used in the 1932 survey has gone out of use; it is necessary, however, that any future surveys whose results are to be compared with the 1947 survey should use the Terman Merrill Binet, even though further Binet revisions appear in the interval. Dr. Fraser Roberts does not find it necessary for comparisons of intelligence only

to use the same group test on all occasions. But if the relationship between intelligence and social conditions, using the 20 per cent sample of the population, is to be compared over intervals of time, it becomes equally essential that the group test remain the same. It is in this respect that the construction of some form of "culture-free" group test reserved for population work becomes of importance. It does not appear essential to survey the whole population if the selection of the 1·5 per cent and 20 per cent samples is done with care. The method of selection in the Scottish Mental Survey was by date of birth, the Six-day sample who were individually tested being those children born on the first day of the even months, and the Thirty-six day sample, for whom a more detailed sociological record was compiled, were those born on the first three days of each month. The Thirty-six day is virtually a 10 per cent sample, and it has been found that in all data which are common to the Thirty-six day sample and the whole survey group of 75,000 or so children, the sample proved representative within the accepted statistical limits. A 20 per cent sample of the population should make assurance doubly sure; and it may be noted that Government Departments, particularly the Ministry of Labour, have obtained satisfactory results with carefully selected samples of as low as 1 per cent. A sample of 20 per cent, therefore, might be expected both to satisfy Dr. Fraser Roberts's requirements for comparisons of average intelligence, and to provide sufficient data for comparisons of the sociological and intellectual characters of the population.

We have advocated for surveys involving intelligence measurement the consistent use of the same tests of intelligence. There is the same need for uniform practice in recording sociological data. Certain interesting comparisons of social conditions in Scotland have been found impossible owing to the fact that different surveys have recorded the same basic data in different forms. In recording the degree of overcrowding in the home, for instance, a man and his wife with a child aged eight and

another aged four may be counted as four persons, two persons and two half persons, three and a half persons, or two and three-quarters persons, according to the form of classification adopted. If no uniform system of publishing the raw basic data is adopted comparison between one survey and another is impossible, and what may be more serious, comparisons may be made between data which appear to be the same, but which in fact are not. An agreed form of publication of original data would enable results from different sources to be dovetailed together, and would serve the purpose for trends in social conditions as consistent use of the same intelligence tests would do for trends in national intelligence.

It would appear likely that future studies in the development of the quality of the population will lie in both the sociological and the genetic fields. Genetic changes in a human population occur slowly, and studies must extend over several generations. It is of importance that the foundations we are laying for the studies of our succesors should be sound.

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XVI

PERSONALITY TESTS AS RESEARCH TOOLS

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OBJECTIVE TESTS OF PERSONALITY

In the last decade, great advances have been made in the practice and theory of personality assessment. The demands of the Services for psychological tools which would aid in the detection of the potential leader or the man liable to break down under stress, provided the stimulus for research and the opportunities for fruitful collaboration between psychiatrists and psychologists.

This paper is concerned with the advances that have been made in personality assessment by means of tests, and is further limited to a consideration of objective personality tests only. These are tests where the subject is asked to perform some task under standard conditions and where the response can be objectively measured. Examples are tests of motor co-ordination, tests of speed and of accuracy of response, tests of suggestibility, of persistence, of level of aspiration and tests of expressive movement. As measure of an individual's persistence, the length of time is noted that he continues with a task despite physical discomfort or despite boredom. Suggestibility is measured by means of the body sway test. The subject is asked to stand quite still while listening to repeated suggestions that he is falling forward all the time. His suggestibility score consists of the number of inches that he sways in response to the suggestion. Level of

aspiration tests provide an indication of the individual's ambition and of his assessment of his own abilities. The subject is given some motor task; after a few trials, sufficient to familiarize him with the difficulties of the task, he is asked to estimate how well he will perform on a subsequent trial. The discrepancy between the estimated future score and the actual performance score of the preceding trial is taken as a measure of the subject's level of aspiration.

These tests measure characteristics of behaviour, the way an individual sets about a task. Although such behaviour is indicative of the deeper levels of the psyche, objective personality tests do not tap these as specifically as do projective personality tests. Another difference between these two types of tests is that the results of the objective tests are not affected by the psychologist's subjective interpretation of the responses given. Such interpretation is necessary in the case of the projective tests and so introduces an additional unknown variable.

The decision to discuss objective personality tests in preference to the more widely known projective tests was made for two reasons. First, because the advances in this field have been made largely in this country, notably by Dr. Eysenck and his collaborators at the Institute of Psychiatry, London. The second reason is rather a different one. It seems that objective personality tests have occupied for some time a Cinderella position in the field of personality assessment. They have been classed as sterile laboratory tests, too much removed from "the inner workings of the human mind", and have often been compared unfavourably with their more glamorous and dynamic cousins, the projective tests. In this paper, an attempt will be made to correct this view, first by pointing to the very important role that these tests have already played in studying problems in the field of clinical psychology and of genetics, and secondly by indicating the contribution that such tests might make to the study of problems pertaining to the field of social psychology.

FACTORIAL ANALYSIS AND TEST DEVELOPMENT

The development of the objective personality tests went on side by side with the application of the statistical method of factorial analysis to the field of personality. In such studies, Eysenck isolated two factors of personality of which he identified the first as a factor of neuroticism or of emotional instability. This factor, Eysenck conceives as a personality variable ranging from the extremely stable, mature, through the average sort of personality to the extremely unstable, neurotic type of personality. In a study which the author carried out together with Petrie and Desai, it was shown that this factor of neuroticism can be operationally defined in terms of responses given to a variety of objective tests. The majority of these tests were specially designed for the enquiry. Classification of our subjects on the basis of their test responses alone was successful in 80 per cent of the cases. Lack of emotional stability showed itself at all levels of behaviour; in poor sensory and motor co-ordination, in slowness at starting, in lack of staying power and in a high degree of suggestibility and of rigidity.

In subsequent work, the range of tests was increased and resulted in the development of a battery of tests which proved an effective measure of the degree of neuroticism of an individual. The work was further extended to a comparative study of normal and of neurotic children. New tests were devised, sampling as far as possible those behavioural characteristics which had been found to correlate with neuroticism in adults. The results on children between nine to fourteen years of age, again showed that 80 per cent of the children could be correctly classified by means of these tests. The test pattern of neurotic children was found to be similar to that of neurotic adults. An 80 per cent correct classification is a very promising result if one bears in mind that the normal control groups had not been psychiatrically screened. According to results obtained by Fraser, 5 to

10 per cent of any unselected normal population will, on examination, be found to be neurotic.

Tests have also been developed to measure the second factor isolated by Eysenck, namely that of extraversion—introversion or its neurotic counterpart, hysteria—dysthymia.[1] It was found, for instance, that anxious and depressed patients, compared with hysterical patients were more persistent, were accurate rather than quick, and had higher levels of aspiration while at the same time underrating significantly more the quality of their actual performance. This description of the two types of patients will be found to be similar to that commonly presented in psychiatric textbooks.

It is of interest in this connexion that the suggestibility test which according to all psychiatric theory should discriminate well between the two types of disorders, failed to do so. Charcot and others subsequently have described hysteria as a condition where the patient is in a state of heightened, even excessive suggestibility. It was therefore to be expected that hysterics would have a higher suggestibility score than other types of neurotics. However, no such differences were obtained and it was found that this test provides a measure of *degree* and not of *type* of maladjustment, i.e., the more seriously disturbed the patient, irrespective of his symptomatology, the more suggestible he is. Such findings are of considerable theoretical importance in showing that it is necessary to speak of different types of suggestibility rather than of some kind of general overall suggestibility as has commonly been the practice. Once these different types have been described by psychiatrists, tests can again be devised to examine their distribution within the neurotic population.

The above discussion provides an example of the type of problem that can fruitfully be investigated by means of objective personality tests. Three further examples will be given of which the first two deal with problems in clinical psychology.

[1] Dysthymic patients are patients suffering from anxiety states and/or depression.

NEUROTICISM, PSYCHOTICISM AND NORMALITY

The first is concerned with the relationship between the normal, the neurotic and the psychotic states. Kretschmer maintains that these conditions differ only quantitatively not qualitatively from one another and that they can therefore be viewed as lying along the same continuum with the two main types of psychosis, namely schizophrenia and manic depressive psychosis forming the two extreme ends of the continuum. Other psychiatrists hold that while the normal and the neurotic differ only quantitatively from one another, they differ qualitatively from the psychotic.

Eysenck, using a wide range of objective personality tests showed that, while the responses of normal and of psychotic subjects differed significantly on most tests, the patterning or intercorrelations of the tests was sufficiently similar for the two groups to lend strong support to Kretschmer's hypothesis that the difference between the normal and the psychotic state is a quantitative one.

Eysenck further showed that it is highly unlikely that all three states, the normal, the neurotic and the psychotic lie along the same continuum since many of the tests which discriminate significantly between normal and neurotic subjects failed to discriminate between normal and psychotic subjects and vice versa. Thus, normal and psychotic subjects gave very similar scores on tests of persistence, of perseveration and of suggestibility. The last mentioned test had been shown to discriminate best between normal and neurotic subjects. These results would suggest that neurotic and psychotic states lie along different continua and that a dimension of psychotic predisposition would have to be postulated, in addition to and probably quite independent of, the dimension of neurotic predisposition.

A comparison of the results of normal, schizophrenic and manic depressive subjects showed that the scores of the schizophrenic groups tended to lie somewhere between those of the

normal and the manic depressive groups. "In other words, it would appear as if we were dealing with one continuum, ranging from normal through schizophrenic to manic depressive."

This study, using objective personality tests, has provided a body of data which should prove of great importance in the clarification of the relation of normal, neurotic and psychotic states to one another. Such studies are of necessity largely exploratory in character and often suggest problems rather than provide definite answers. They show, however, beyond doubt that objective personality tests, which, unlike psychiatrist's ratings and projective personality tests, are unaffected by the theoretical viewpoint of the investigator, provide an effective means by which psychiatric theories can be submitted to experimental verification.

OTHER APPLICATIONS OF TESTS TO CLINICAL PROBLEMS

Another problem of clinical psychology studied with the help of such tests is that of the effect of the leucotomy operations upon personality. Petrie gave tests to patients before, and at stated intervals after, leucotomy. Inspection of the tests on which significant differences were found enabled her to describe the characteristics of the post leucotomy personality and so to contribute to our knowledge of the effect of these operations and with it to that of the function of the frontal lobes. The tests also proved sensitive enough to show differences in the effects of various types of leucotomy operations. Enquiries of this kind could be extended to less drastic forms of therapy. They would provide a useful objective measure of change in the patient to be related to the psychiatrist's evaluation of the effects of therapy and to the patient's own assessment of improvement.

The last study to be mentioned links the field of clinical psychology with that of genetics. Eysenck and Prell used the battery of personality tests for children previously referred to, to determine the extent to which neurotic predisposition is geneti-

cally determined. Using the well-known twin method of comparing similarity of test responses of same sex fraternal twins with that of identical twins, they found (with teen-age children as subjects) that the intra-class correlation coefficient on tests of neuroticism was 0.22 for fraternal twins and 0.85 for identical twins. This indicates clearly that neuroticism, as defined by these tests, is no statistical artefact but a biological entity which is largely genetically determined. These findings have far reaching implications. It should soon be possible to produce correlation coefficients on familial resemblance in the field of personality comparable to the well-known ones which have been obtained in the field of cognition.

APPLICATIONS OF TESTS TO SOCIO-PSYCHOLOGICAL PROBLEMS

Passing now more specifically to the field of social psychology, we can point to beginnings rather than to results already obtained. In a research which the author is carrying out at present into the differences in attitudes and personality characteristics of young adolescents coming from different social classes, objective personality tests will be given. They will serve to test the hypothesis put forward by Davis of Chicago concerning the effect of social class membership upon the personality of the children. Davis suggests that middle-class parents, relative to their status, have higher aspirations for their children than working-class parents, that they try to implant in the child a desire to get on in the world and to teach him attitudes and behaviour consistent with such striving. The child tends to introject the demands of the parents and to react to them by having high aspirations in his turn and by being anxious regarding the attainment of these aspirations. It is proposed to test this hypothesis by seeing to what extent middle-class children compared with children from other classes have significantly higher scores on tests of level of aspiration and on those indicative of anxiety. The relative independence of these

tests from intelligence makes them specially suited for investigations of this kind.

Another area of social psychology where these tests could usefully be applied is that of the study of national characteristics. It would be of interest to determine whether the various nationals score more highly on those tests which measure behavioural characteristics which are commonly attributed to them. For instance, will the Americans be found to have a higher level of aspiration score, the British a higher persistence score and the French a higher score on tests of impulsiveness? In general, will there be greater differences between subjects belonging to the various social classes of any one country than between subjects from a cross-section of that country and that of another country? The tests would serve to provide answers to these questions and so throw light upon the relative effect of social class and national membership.

Of special interest in this connexion would be a comparative study of the personality characteristics of such deviant groups as delinquents and neurotics in the different countries. For investigations of this type, objective personality tests are specially useful since they are relatively culture free in their administration, and since they give a measurable response which does not depend upon subjective interpretation by the psychologists, which always introduces the danger of reading our culture norms into the responses given by subjects from other cultures.

In this paper, examples from the field of clinical psychology and of genetics have been given to show the important role that objective personality tests have played in the elucidation of significant problems. An attempt has also been made to indicate why it was thought that the application of objective personality tests to problems of social psychology would prove rewarding.

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XVII

SOCIAL ATTITUDE RESEARCH

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PUBLIC OPINION SURVEYS

THE great interest that is taken in social attitude research by many thoughtful people is shown most vividly by the success of such organizations as the Gallup Poll and similar commercial ventures; equally indicative of the value placed on social psychology is the official support given to such work as the government-sponsored Social Survey. It would be erroneous, however, to consider these bodies as being primarily concerned with the furtherence of social science; their main function is the application of techniques and methods worked out by psychologists, statisticians, and sociologists to a variety of practical problems, such as the prediction of election results, or the assessment of the effects of an educational campaign. In so far as they adhere to the strictest standards of sampling, interviewing, and statistical treatment of the data, the results of their work are of outstanding practical importance; one's only regret must be that far too little use is being made of the services such organizations can render. Comparison with the very impressive volume of work done in the United States of America along these lines shows that such a view can easily be supported by factual evidence; as a single example we may quote the work done for the American Army by the Research Branch, Information and Education Division, under S. A. Stouffer.

It would be erroneous, however, to imagine that work of this type is characteristic of social attitude research, or that indeed the results of it can be said to contribute much to our scientific knowledge in this field. Occasionally, indeed, valuable generalizations emerge from the results collected by polling agencies; as an example we may quote the American observation that in the domestic field there is a tendency for the opinion of the poorly educated sections of the population to prevail, while in the field of foreign policy the well educated appear more likely to carry the day. But by and large, the results emerging from the work of the polling organizations are isolated, fragmentary, and unrelated; the questions which they seek to answer are dictated by practical necessity, rather than by scientific curiosity, and the answers are unlikely to be of fundamental scientific importance.[1]

THE RELATION OF ATTITUDES AND PERSONAILTY

We may exemplify the importance of the interrelatedness of scientific facts by quoting the pioneering work of S. Crown, to which attention was drawn at the Brighton meeting of the British Association. Like many others before him, he studied the incidence of anti-semitism in the population, using specially prepared questionnaires on a suitably chosen sample of the population. If he had stopped there, the result would have been just another isolated fact, of some interest but of little scientific value. He went further, however, and elaborated the hypothesis that anti-semitism is in part caused by a set of factors which may be identified as "emotional instability" or "neuroticism". Using a measuring instrument for this factor of emotional

[1] Other reasons why polling data are of less interest to science than might have been hoped at one time are related to the almost invariable practice of polling organizations to use direct questions which admit only of a "Yes"—"No" answer choice. This rules out somewhat more indirect approaches, such as the investigation of stereotypes for instance, and it does not take advantage of recent advances in the construction of scales for the measurement of social attitudes. There and other criticisms are discussed at some length elsewhere (cf. McNemar and Eysenck & Crown).

instability which he had himself constructed and carefully validated, he showed that there was a highly significant correlation between a person's score on the anti-semitism questionnaire, and his score on the emotional instability measure. By thus confirming his original hypothesis, and showing that emotional instability is characteristic of the anti-semite, Crown laid the foundation for a more scientific understanding of the causes of anti-semitism. The more recent American work on this subject, published under the title of "The Authoritarian Personality" by Adorno *et al.*, has thrown additional light on this subject, and has in the main supported Crown's interpretation.

If *interconnectedness* of data is one desideratum of scientific observation, *invariance* of results is another. If we carry out a study on male adolescents in Surbiton, we would consider our results of little interest if they failed to be borne out by another study of maiden aunts in a little country village. At the very least, if we discovered that invariance was lacking we would demand to know the exact relationships existing between the variable we were interested in (the dependent variable), and such independent variables as age, sex, residence, social class, nationality, education, and so forth.

THE ORGANIZATION OF SOCIAL ATTITUDES

Social attitude research in this country, in so far as it has been concerned with fundamental problems involving interconnectedness of data and invariance of results, has dealt in the main with the problem of the structure or organization of social attitudes. The early work of Thurstone, Ferguson, Lentz, and Nelson had left little doubt that social attitudes are not independent of each other, but are related in various complex ways. Indeed the fact that political parties are able to exist, and draw up programmes containing a number of different proposals which presumably are expected to find favour with the majority of adherents of that party, suggests that some at least of the principles according to

which this organization or structure takes place are understood and made use of by politicians. Discussion of these putative principles by social scientists and politicians, however, shows marked divergences in interpretation. It is customary, for instance, to maintain that it is possible to arrange individuals (and parties) along a continuum from "left" to "right", going from communist through socialist to liberal, and on from liberal through conservative to fascist. Quite contrary to this interpretation, though often held by the same people, is the view that there is little to choose between communists and fascists, and that both are at the other end of a continuum to the democratic parties. Liberals, in particular, often hold this latter view, sometimes going even further and suggesting that it is they who form the true opposite to the "dictatorship" parties, and that socialists and conservatives would be found intermediate between themselves and the communist-fascist block on this hypothetical continuum.

A comprehensive review of American work to date, and the analysis of extensive data gathered in this country by Flugel and Hopkins, suggested to the writer that both these seemingly contradictory views might be correct, and led to a lengthy series of researches devoted to the proof or disproof of a formal hypothesis regarding the organization of social attitudes. This hypothesis starts with the assumption that both the views outlined in the preceding paragraph are correct. It follows from this that it will be impossible to represent the structure of attitudes in one dimension, and that at the very least two dimensions will be required. The resulting picture may be represented in diagrammatic form, as in Fig. 13. The abscissa represents the radical-conservative continuum, and it will be seen that the five parties represented come in the order communist—socialist—liberal—conservative—fascist. The ordinate represents the hypothetical continuum along which the parties would appear in the order: communist-fascist, socialist-conservative, liberal. In this way we would account in one single hypothesis for all the suggested interrelations.

Proof for this hypothesis was attempted along a series of different lines. In the first study, a forty-item questionnaire covering a large variety of different social attitudes was given to 750 respondents, with instructions to endorse or reject each statement

FIG. 13.—Position of Political Parties in Two-Dimensional Space

(cf. Appendix). The subjects were carefully chosen so that 250 came from each of the major parties (labour, liberal, conservatives), matched for age, sex, and education. Responses to the forty statements were then intercorrelated. The table of intercorrelations disproves conclusively two possible hypotheses either of

which would effectively contradict the one under discussion. It was shown (i) that attitudes are not unrelated; the correlations are considerably in excess of zero. It was also shown (ii) that one factor is not sufficient to account for all the intercorrelations; the rank of the correlation matrix is higher than one. A factor analysis was carried out of the matrix, and two main factors extracted. These factors are plotted in diagrammatic form in Fig. 14.

It will be seen that the abscissa represents the hypothesized radical-conservative factor. One extreme of this continuum is characterized by attitudes such as: Nationalization is inefficient, general strikes should be made illegal, "My country right or wrong", unemployment is the only incentive which will make people perform disagreeable jobs. The other extreme is characterized by attitudes such as: Private property should be abolished, the laws favour the rich, give up national sovereignty to preserve peace. This interpretation of the factor was given additional support by showing that those items which had the highest factor saturations (i.e., were most characteristic of this factor) also distinguished most clearly between our samples of avowed socialists and conservatives. There is thus little doubt about the nature of this first factor.

The second factor does not find any ready-made nomenclature to hand which might be used to characterize it. At the one extreme we seem to have materialistic, hedonistic, tough-minded attitudes, such as those calling for euthanasia, compulsory sterilization, and the abolition of laws against abortion, as well as attitudes of superiority towards women, Jews, and coloured people, and an approval of war, compulsory military training, and flogging, and capital punishment. At the other end we have idealistic, ethico-religious, tender-minded attitudes, such as those approving of church-going and religion, pacifism, the curing, rather than the punishment, of criminals, and the equality of sexes and races. Using William James's terms, this factor was called "tender-mindedness *v.* tough-mindedness"; it will be

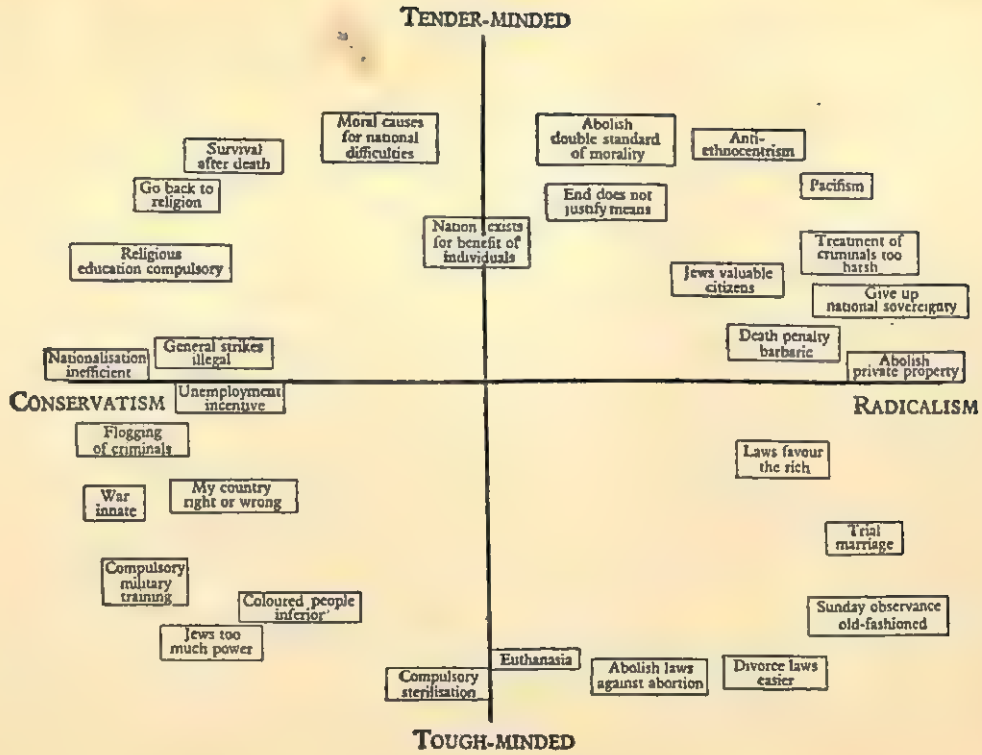

TENDER-MINDED
Moral causes for national difficulties
Abolish double standard of morality
Anti-ethnocentrism
Survival after death
Go back to religion
End does not justify means
Pacifism
Nation exists for benefit of individuals
Religious education compulsory
Treatment of criminals too harsh
Jews valuable citizens
Give up national sovereignty
Death penalty barbaric
Nationalisation inefficient
General strikes illegal
Abolish private property
CONSERVATISM
Unemployment incentive
RADICALISM
Flogging of criminals
Laws favour the rich
War innate
My country right or wrong
Trial marriage
Compulsory military training
Coloured people inferior
Sunday observance old-fashioned
Jews too much power
Euthanasia
Compulsory sterilisation
Abolish laws against abortion
Divorce laws easier
TOUGH-MINDED

FIG. 14

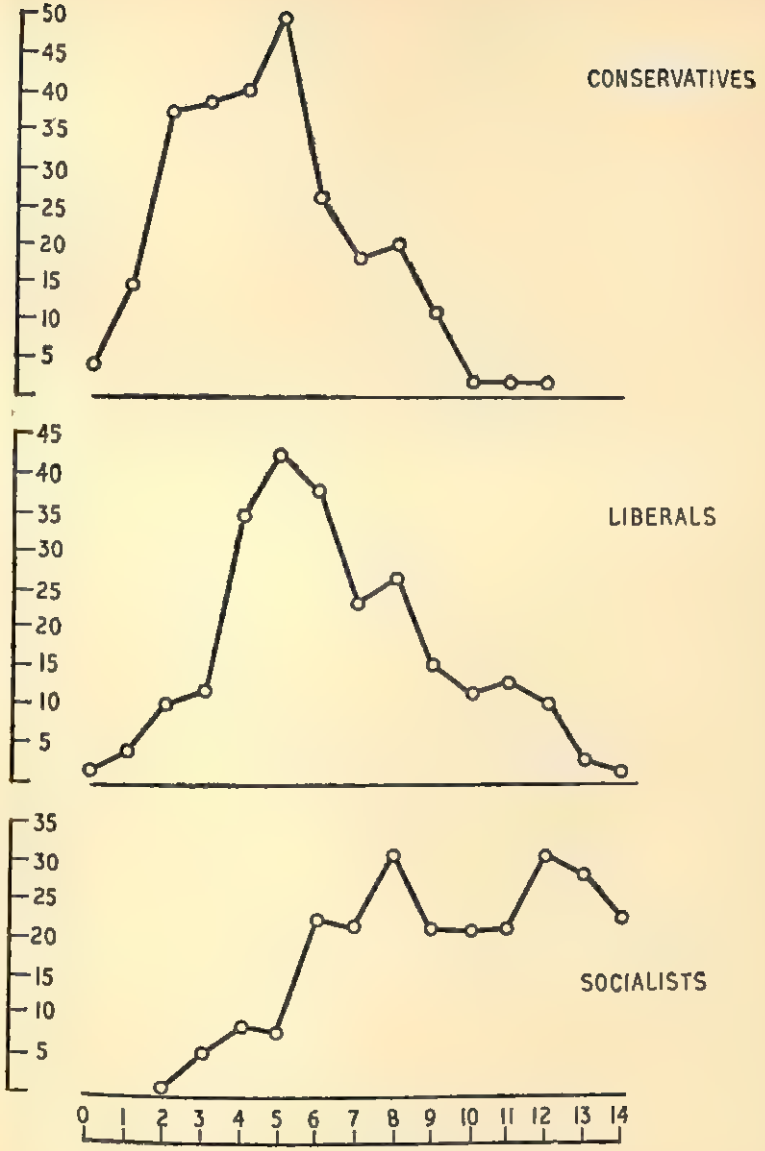
referred to as the T-factor, while the radical-conservative factor will be referred to as the R-factor.

THE VALIDITY OF RADICALISM AND "TOUGH-MINDEDNESS" ATTITUDE SCALES

The structure of attitudes shown in Fig. 14 is roughly in conformity with our hypothesis, but some more direct method is required before we can accept its validity. Scales were therefore constructed of both the R and T factors, made up of those attitude statements in the questionnaire most highly saturated with these two factors respectively. These two scales were found to have sufficient reliability for group comparisons, and to be intercorrelated to a negligible extent. They may therefore be accepted as useful measuring instruments for the purpose of further investigating our main hypothesis.

Figure 15 shows the distribution of three groups of 250 respondents on the R-scale. These groups matched for sex, age, and education, were made up of conservative, liberal, and socialist voters respectively, and it will be seen that while there is much overlap, the mean scores of the three groups are significantly differentiated. On a fifteen-point scale, the means of the groups are respectively 4·6, 6·3, and 9·4. Later studies have verified again and again that the R-scale can with high statistical significance differentiate groups of people known to have voted for different political parties. One such study compared the R-scale scores of communists and fascists with the values reported above for the three democratic parties. Communists were found to have a score of 12·4, which puts them well to the left of the socialist group; fascists were found to have a score of 5·2, which puts them close to the conservative group. With respect to the ordering of the five parties along the radicalism-conservatism continuum, we may therefore say that the results bear out our prediction.

It is more difficult to find external validation groups for the T-factor. As the existence of this factor had not been recognized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CONSERVATIVES
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LIBERALS
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SOCIALISTS
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FIG. 15

prior to its experimental establishment, no organized groups could be found which would be acceptable as indisputably "tough" or "tender". Two possible validation groups were considered and subjected to empirical study. It was hypothesized that men would be more tough-minded than women, and that working-class groups would be more tough-minded than middle-class groups. (The original hypothesis that fascists and communists would be found to be more tough-minded than liberals, conservatives, and socialists cannot, of course, be used to establish the validity of the scale used for measuring the T-factor without involving us in a circular argument.) Marked differences in the expected direction have always been found as between men and women, equated for age, education, and political party adherence; we may thus regard this result as supporting our interpretation.

The hypothesis that working-class groups would be found to be more tough-minded than middle-class groups was tested by comparing working-class communists with middle-class communists, working-class socialists with middle-class socialists, working-class liberals with middle-class liberals, and working-class conservatives with middle-class conservatives. Fig. 16 shows quite clearly that in each case the working-class group is more tough-minded than the corresponding middle-class group. (It will also be seen that in each case the working-class group is more conservative than the corresponding middle-class group. There is no space in this brief summary to discuss the reasons for this somewhat surprising finding.)

PATTERNS OF ATTITUDES IN POLITICAL, NATIONAL AND OTHER GROUPS

We have shown, then, that in so far as validation studies can be attempted they tend to show the validity of the T-scale. We must now turn to our main hypothesis and compare the T-scores of the different political parties. In the first comparative

study carried out by the writer, the T-score of the conservative group was 7·6, that of the liberal group 7·9, and that of the socialist group 8·0, making the socialists and liberals somewhat more tender-minded than the conservatives. An unpublished recent study by D. Melvin has given the values of 7·3, 8·2, and

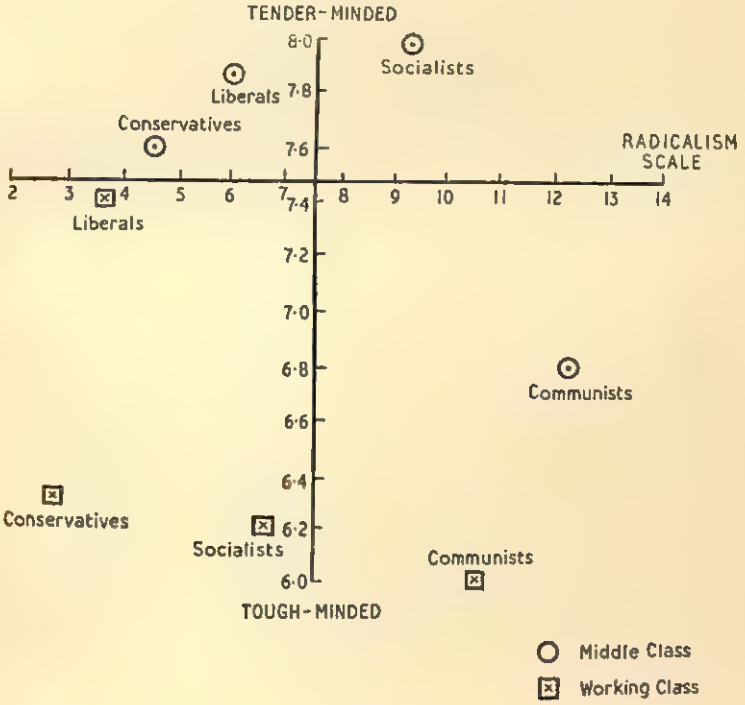

FIG. 16

7·3 for groups of conservatives, liberals, and socialists respectively; here the liberal group is clearly and quite significantly the most tender-minded. (The possibility cannot be ruled out that in the six years elapsing between the two studies there has been a tendency for the more tender-minded socialists to leave the Labour Party and to vote instead for the Liberal Party.) In a third study,

comparing working-class groups, scores on the T-factor were 6·3, 7·4, and 6·2 respectively, again putting the liberal group as the most tender-minded. When we come to the communist and fascist groups, we find considerably lower (tough-minded) scores. Middle-class and working-class communists had scores of 6·8 and 6·0 respectively, while fascists had an all-time low score of 4·7.[1] These results, then, strongly confirm our original hypothesis regarding the two-dimensional organization of social attitudes as represented in Fig. 13.

It should be possible to make further predictions in the political field in terms of the R and T factors. Thus members of the I.L.P., of the Society of Friends, and pacifist groups would be likely to be found in the tender-minded radical quadrant, while members of the Salvation Army, and other religious bodies would be found in the tender-minded conservative quadrant. Such predictions admit of an easy and obvious empirical check.

So far we have dealt with the interrelatedness of social attitudes; we must now turn to the invariance of the pattern discovered. It has been shown in unpublished work that age, sex, and education do not distort the pattern in any way; surprisingly enough, even intentional distortion through biased sampling appears to have little influence on the emergence of the pattern. Thus in one analysis, correlations were run on the forty-item questionnaire for conservatives only, for liberals only, and for socialists only; the resulting three matrices were then submitted to factorial analysis, or rather that special form of it called criterion analysis. The R and T pattern was observed again, almost completely unchanged in spite of the deliberate sampling bias.

But of much greater interest than these studies of invariance within one country are some investigations carried out in an attempt to compare the British pattern with that which might be found in other countries. In an unpublished study, A. Conway and the present writer repeated the work reported here with an

[1] The number of fascists included in this study was too small to allow of any break-downs according to social class.

American group of subjects living in Pennsylvania, and showed that in almost every detail the results from the British investigations could be matched in the American study. This similarity extended not only to the actual pattern of intercorrelations, but also to the correlation of R and T with outside variables. Thus in one study in this country, it has been shown that "social insight", as measured by a specially constructed test, was positively correlated with radicalism, so that on the average radicals tended to show more social insight than did conservatives, regardless of education. Exactly the same relation was found in America, and indeed the correlation was almost identical with that observed in this country.

Similarly, work done in Sweden by Nilsson, Dahlström, Husén, and others shows that there also the analysis of the interrelations of social attitudes into R and T is applicable, and gives results closely approximating those found here. And lastly, an unpublished study by the present writer shows that in Germany, too, the pattern of organization of social attitudes is very similar to that obtaining in Great Britain, the United States, and Sweden. This invariance is not complete however; thus, for instance, attitudes of anti-semitism have much higher correlations with conservatism in Germany than they have in this country. Such differences, however, are relatively few, and are usually easily understood in terms of definite historical causes. On the whole, invariance of the R-T pattern is much greater than seemed likely on *a priori* grounds at the beginning of the experimental study.

The research described in this paper may be said to have laid a firm foundation on the descriptive level for the study of social attitudes. As long as it is realized that the factors discussed are unlikely to be the only ones active in the structuring of the field, and as long as the fact is kept in mind that we are dealing throughout with tendencies, averages, and probabilities, future work is unlikely to change our picture to any considerable extent. Unfortunately, however, very little is known about the equally

important problem of causation. We are unlikely to accept the Gilbertian belief

> that every boy and every gal
> that's born into this world alive,
> is either a little Liberal,
> or else a little Conservative,

if only because environmentalistic explanations are more in fashion nowadays. But so far we have very little to put in its place in the way of accounting for the origin of the attitude pattern observed. An hypothesis which has received a certain amount of support by American writers like Lasswell, Krout and Stagner, and Rundquist and Sletto, and which would link radicalism with emotional instability and neuroticism, has been shown fairly decisively to be quite inapplicable in this country at least; there is no tendency for socialists to be more unstable than conservatives or liberals. It may be surmised that the coming years will see a rapid growth of interest in this problem of causation, and as the measuring instruments for R and T already to hand are probably quite adequate, in spite of certain imperfections, for the initiation of such experiments it is the writer's hope that in a few years' time another report to the British Association on social attitude research may contain many references to empirical work on this vital problem.

APPENDIX

INVENTORY OF SOCIAL ATTITUDES

Below are given forty statements which represent widely held opinions on various social questions, selected from speeches, books, newspapers, etc. They were chosen in such a way that most people are likely to agree with some, and to disagree with others. After each statement, you are requested to record your personal opinion regarding it. If you strongly approve, put two crosses after it—like this: + +.

If you approve on the whole, put one cross after the statement. If you can't decide for or against, or if you think the question is worded in such a way that you can't give an answer, put a zero—like this: O. If you disapprove on the whole, put a minus sign. And if you strongly disapprove, put two minus signs, like this: – –. Be sure not to omit any questions.

Attitude Statements	*Your opinion*
1. Coloured people are innately inferior to white people.	.....
2. Present laws favour the rich as against the poor.	.....
3. War is inherent in human nature.	.....
4. The marriage bar on female teachers should be removed	.....
5. Persons with serious hereditary defects and diseases should be compulsorily sterilized.	.....
6. Our treatment of criminals is too harsh; we should try to cure, not to punish them.	.....
7. Our present difficulties are due rather to moral than to economic causes.	.....
8. In the interests of peace, we must give up part of our national sovereignty.	.....
9. Sunday-observance is old fashioned, and should cease to govern our behaviour.	.....
10. It is wrong that men should be permitted greater sexual freedom than women by society.	.....
11. Unrestricted freedom of discussion on every topic is desirable in the press, in literature, on the stage, etc.	.....
12. Ultimately, private property should be abolished, and complete socialism introduced.	.....
13. Conscientious objectors are traitors to their country, and should be treated accordingly.	.....
14. A certain amount of sex education should be given at school to all boys and girls.	.....
15. The laws against abortion should be abolished.	.....
16. Only by going back to religion can civilization hope to survive.	.....
17. Marriages between white and coloured people should be strongly discouraged.	.....

Attitude Statements	*Your opinion*
18. Jews are as valuable, honest, and public-spirited citizens as any other group.	.....
19. Major questions of national policy should be decided by reference to majority opinion (e.g. by referendum).	.....
20. There should be far more controversial and political discussion over the radio.	.....
21. The present licensing laws should be altered, so as to remove restrictions on hours of opening.	.....
22. All human beings are born with the same potentialities.	.....
23. Divorce laws should be altered to make divorce easier.	.....
24. Patriotism in the modern world is a force which works against peace.	.....
25. Modern life is too much concentrated in cities; the government should take steps to encourage a "return to the country".	.....
26. Crimes of violence should be punished by flogging.	.....
27. The nationalization of the great industries is likely to lead to inefficiency, bureaucracy, and stagnation.	.....
28. It is right and proper that religious education in schools should be compulsory.	.....
29. Men and women have the right to find out whether they are sexually suited before marriage (e.g. by companionate marriage).	.....
30. The principle "Spare the rod and spoil the child" has much truth in it, and should govern our methods of bringing up children.	.....
31. Women are not the equals of men in intelligence, organizing ability, etc.	.....
32. Experiments on living animals should be forbidden.	.....
33. The Jews have too much power and influence in this country.	.....
34. Differences in pay between men and women doing the same work should be abolished.	.....
35. Birth control, except when medically indicated, should be made illegal.	.....
36. The death penalty is barbaric, and should be abolished.	.....
37. There will be another war in twenty-five years.	.....

Attitude Statements	*Your opinion*
38. Scientists should take no part in politics.	.....
39. The Japanese are by nature a cruel people.	.....
40. Only people with a definite minimum of intelligence and education should be allowed to vote.	.....

Personal Details

It would be appreciated if you would fill in the following details.

41. Age. .....
42. Sex. .....
43. Weekly income (self or husband). .....
44. Type of work. .....

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XVIII

SCIENTIFIC TASKS FOR THE PSYCHOLOGICAL CLINIC

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THE UNIQUE OPPORTUNITIES IN THE CLINIC

I SHOULD like to begin by referring briefly to some aspects of the work in a psychological clinic which would appear to have a unique role in contributing to our general understanding of human behaviour. In a medical clinic, the welfare of the individual is naturally the paramount consideration, but I do not wish to refer to the therapeutic aspects of clinical work to-day; and when I speak of therapy in the following remarks I am introducing it in connexion with its place in the scientific goals of general psychology.

Let us look for a moment at what goes on in an out-patient psychological clinic such as the one in which I work. Individuals in distress come to other people for relief. They come with *needs* to ease painful inner tensions and with the hope that they will at last find someone who will help them to satisfy these needs. Psycho-analytic experience has shown that almost the whole gamut of the human needs and strivings that underlie everyday behaviour are brought into this therapeutic relationship in our patients' demand for help. Formerly, in regard to clinical work, a sharp line was drawn between normal and abnormal behaviour. However, although manifestations of pathological behaviour may appear different in kind from what we ordinarily think of as

"normal", all clinical experience confirms the view that quantitative differences in the basic needs common to all individuals account for the qualitative differences on the surface.

The needs that create and maintain human relationships are the central problem in the psychology of personality. It would be reasonable, therefore, to expect that an understanding of needs and a useful theoretical framework for the psychology of personality and behaviour would come only when the psychologist could study these forces under conditions in which he could see real needs in action and in interaction with the objects they seek. The theurapeutic need-relationships sought by patients create a unique opportunity to study these central features of human behaviour. Not only do they present needs of a wide range, both in kind and intensity, but the psychologist in the role of therapist is himself the dynamic focus in the situation sought by the subject. This other person in the relationship, the therapist, must, of course, play a special role because a relationship is a two-way process. I hope that it will become clear that in this other role, it is only when the therapist has an adequate understanding of the laws governing behaviour that he is able to help the patient. Only then can he remove those irrational conflicting needs which bring the patient to treatment and which also prevent him from achieving satisfaction with the normal objects available to him in his outside life. It is the diminution of his distress that makes the therapeutic relationship one of real significance to the patient; and for the psychologist, there must be in addition to the patient's relief, the understanding of what has happened.

Let us take a simple example. If we can study in the clinic a problem such as jealousy in the patient's interactions with the therapist, with all its intricacies and implications, its origins and its effects on the individual, then we can learn a great deal about an everyday problem in many relationships, and with much richer data than could be obtained by some less direct approach such as questioning the patient about his tendencies to exhibit this behaviour. In the clinic in which I work, the fact that a member

of the staff is seeing a patient and should not be disturbed is indicated in the usual way by a prominent label with the word ENGAGED hung on the door. When Mr. A.'s jealousy over Miss B. getting attention suddenly makes him on one afternoon secretly turn the card round during her session in the hope that it will be interrupted, then we can study what in fact is a representative sample of the dynamics of the eternal triangle.

CONCEPTS REQUIRED

Taking the great variety of behaviour exhibited by patients in their needs, in the various attempts they make to establish all kinds of relationships with their therapists, what kind of concepts do we require if we are to describe and understand all these interactions, and if we are to create a framework for the building of an adequate body of knowledge about human relations and behaviour?

At least the following would appear to be necessary:

1. *Concepts about needs and goals.* Just as gravitation is a basic concept in mechanics, the basic concept of most use in the explanation of human behaviour is that of needs and goals. People need objects to diminish the tensions set up within them. Concepts of this kind have been used for long enough by psychologists, e.g., in the theories of Freud and McDougall. My colleagues and I have found the most useful way of formulating these concepts in Lewin's work.

A need may be one of the basic biological needs such as hunger, or it may be a secondary or derived need, e.g., to secure a certain position in society which guarantees to the individual the secure supply of what he needs for his basic biological functions. Again, a need to engage in some artistic or recreational activity may exist to deal with a situation in which unconscious anxiety is aroused.

In all these cases, the need concept is the basic notion we require to provide the underlying motive power.

2. *Concepts about forces in the external field.* The need-goal concept, however, is not enough by itself to explain the various ways which the individual adopts in different circumstances to meet the same needs. We must introduce other concepts to explain why changes in the psychological environment produce the changes they do, for instance, why the behaviour of a patient in a therapeutic group alters according to whether or not a man whom he fears is present, or why the child trying to get a toy from a weaker child may alter its behaviour according to whether an adult is present or not.

3. *The concept of the internal environment.* The effect of the external environment in determining the particular way the individual chooses to find gratification for his needs is still not enough. We have also to introduce another environment, the unconscious or internal environment of the individual to explain why, for example, despite the constancy of the external environment provided within the therapeutic situation, Miss A. on one day may freely criticize the therapist, or threaten to destroy his furniture, in order to get more frequent therapeutic sessions; or on another day she may try to achieve the same goal by ingratiating behaviour. Again, on a third day, while displaying a greater need than ever for help, she may struggle against the wish for more treatment by telling the therapist that she is getting more than her share and that he should therefore take a well-earned holiday.

This concept of an internal environment, or "private world" is even more obviously required to account for the more markedly irrational aspects of behaviour as in phobias or obsessions, or in the everyday illogicalities that all of us show in many of our activities.

Put in another way, it is these different private worlds which all of us carry within ourselves which account for the fact that we perceive and assess the same external environment situation in different ways; and it is the mobilization of different parts of this private world in the course of treatment, the dominance at

different times of various unconscious fantasies, which accounts for the fact that the patient sees the unchanging external therapeutic situation in a different light from session to session and therefore chooses different behaviour patterns in his desire to satisfy the same needs.

4. *Concepts about the dynamics of the role of the therapist.* Further evidence about this unconscious private fantasy world can be provided by the effects of the therapist's contribution to the therapeutic relationship. For instance, a patient who in her previous session had given ample indications of both loving her analyst and hating him for his lack of response, in one session refused to lie down for fear of soiling the couch with her shoes although she normally put her feet on the couch without any reference to it. She then expressed concern about the analyst's health, hoping he would not become ill, etc. If we assume that these apparently unrelated thoughts expressed by the patient in a temporal sequence stem from a common dynamic source, namely, an unconscious relationship in which she wants to see the analyst harmed, we can understand why the patient's conflicting needs of both wanting to preserve the analyst and to attack him find their manifest expression in the particular behaviour exhibited in this session. The pointing out by the analyst of the unconscious hatred as the reason for the patient's fear of soiling the analyst's couch and her concern about his health was followed by the patient losing this fear and lying down on the couch. The theoretical concept which we therefore introduced to explain the effect of our remarks, the so-called analytic interpretation, is that the making conscious of an unconscious impulse to establish a certain behaviour pattern, the consequences of which the patient feared, enabled the patient to perform a process of reality testing which convinced her of the innocuousness of this impulse. The removal of the unconsciously determined fear thus enabled her to dispense with her need to take such extreme precautions against this impulse.

It is the dynamic interaction consequent upon the therapist's

ability to perceive and handle the unconscious relationships sought by the patient that characterizes the therapeutic relationship. The therapist has to learn to answer the question, what kind of relationships does this patient seek which would account for what he is saying or doing or feeling now in this situation?

It will be recognized too, that it is this interaction which makes it possible for the patient to reveal the relevant parts of his private world and hence to restructure this along less disturbing lines.

THE POSSIBILITY OF A CLINICAL LABORATORY

It will be noticed that in this conception of the therapist's role, no use need be made of hypotheses about the patient's past. In recent years there has been an increasing tendency amongst British psycho-analysts to extend the concept of transference in keeping with a growing interest in the concept of unconscious object-relationships—work in which Klein, Rickman and Fairbairn have played major parts. In the Tavistock Clinic, we believe that the rigorous use of this approach has very great possibilities for increasing our knowledge of the dynamic laws of human behaviour. Moreover, our experience in developing techniques based on it during the last five years suggests that it is as helpful to the patient as any other approach. But in the context of this meeting it has a special importance. To quote from one of my colleagues, Ezriel: "If we therefore consider the study of the patient's unconscious, 'latent' needs to establish certain relations with his analyst as the essential feature of the psycho-analytic method, then any manifest material produced by the patient during a session, like memories, fantasies, even deliberate lies must be looked upon as some kind of conscious idiom to give expression to these needs 'here and now'. Even if it seems to be a memory of a past event it can no longer be taken as valid proof that the event referred to had really happened. As opposed to the common belief (still shared by many psycho-analysts) the psycho-analytic method is therefore not capable of yielding any

conclusive evidence with regard to the genesis of the particular individual personality under treatment, i.e., it is not able to reconstruct an objective picture of his historical past. On the other hand, the transference technique enables us to make use of all the material of a session as evidence of what determined the patient's behaviour 'here and now', i.e., it enables us to study the conditions necessary and sufficient to produce a predictable event during that session."

In other words, my colleagues and I believe that we are developing an approach which permits of what might be termed a true *clinical laboratory*, a place in which experimental work on real human need-relationships can be carried out and in which both the therapeutic and scientific aims can be furthered simultaneously. I need hardly emphasize the fact that this notion means something different from a psychological laboratory located in the clinic but in which the psychologist is restricted to the use of current common test techniques. The contributions of all such methods are required. It is our view, however, that until the psychologist develops his methods, and creates the new ones needed, for the ordering of the observations of the dynamics of behaviour that are to be found only within the therapeutic need relationships, then his work on personality will retain its present static approach with concepts which are essentially normative or classificatory.

SOME LINES OF WORK ON NEED-RELATIONSHIPS BEING DEVELOPED IN THE TAVISTOCK CLINIC

(*a*) *Recording of sessions and "micro-analysis"*. A difficulty hitherto in studying the phenomena of the therapeutic relationship has been the accurate presentation of the data to others. In addition, the therapist himself has had no accurate record which he could scrutinize subsequently. We have, therefore, begun to use some of the new methods of sound recording to make records of psycho-analytic sessions, both individual and

group, for the purpose of submitting them to a process of "micro-analysis", i.e., the systematic consideration of the successive steps in the interactions between the two persons.

(*b*) *Psycho-analytic group therapy.* Stimulated by Bion's work with therapeutic groups, we have during the last few years been developing a rigorous psycho-analytic "here and now" approach for use with therapeutic groups. Groups offer very great possibilities for psychological developments when treated in this way. The relationships available are obviously more varied, for they include those between the members of the group as well as those between the group and the therapist. Various "accessory relationships" can also be studied, i.e., the ways in which an individual uses a relationship with one person to further a relationship with another. Yet, perhaps more important, for the first time it is possible to have psychological observers present in the therapeutic situation who can make independent observations. For instance, during the last few years my colleagues and I have attended each other's groups over considerable periods in order to ensure that we share a common method, and other psychological observers have sat with us in order that we could discuss with them common data.

(*c*) *Different contributions from the therapist.* The usual role for a therapist in a "here and now" approach is to keep his behaviour constant by adopting a passive attitude except for his comments or interpretations along the prescribed lines of showing to the patient in what way his "here and now" behaviour is determined by attempts to fit the analyst into playing the parts of the persons he unconsciously needs. That is to say, the analyst behaves constantly while the patient transfers to his present relationship with him various unconscious relations with objects which are the residues of earlier unresolved conflicts.

Once we know more about the dynamics of the relationships to be studied when the analyst behaves in this constant way, we can alter the situation by having him behave differently, e.g., by giving reassurances as contrasted with interpretations; or he can

make different kinds of interpretation, such as historical versus "here and now", or he might refuse to give any interpretations for varying periods. Some tentative studies in these procedures have been made.

(*d*) *The special opportunities with children.* The way in which the needs of the developing personality are affected by the relations experienced in childhood is being studied directly by bringing the child-patient and his parents into therapeutic relationships. The dynamics of the family situation then become accessible and the child's behaviour can be correlated with a fuller picture of his environment than is usually possible.

The familiar method of control group studies can be used readily in this field. As many of you will know, my colleague Dr. Bowlby and his team in the Child Guidance Department of Tavistock Clinic are investigating the effects on the personality development of young children of separation from their mothers at early ages. Here also, however, it is the establishment of a therapeutic relationship with parents and others concerned with the child based on the psycho-analytic approach outlined that permits access to vital data not otherwise obtainable.

Such studies on special factors enable checks to be made on inferences from work with older patients.

(*e*) *The possibility of measurement.* Although we have done little or nothing in this direction so far we believe that unconscious object relations will eventually lend themselves to quantification by a variety of methods, and we should naturally feel that a very great advance had been made when this becomes possible. The assessment of personality would then in our view take on a new lease of life.

(*f*) *Time-scales for projects.* The elucidation of the complexities in interpersonal relationships is not likely to be a short-term programme and in our view research in this field is to be thought of as requiring projects carried on over many years.

SUMMARY

Our present knowledge of the dynamics of human behaviour is lagging because too few psychologists have the opportunity to study real need-relationships under suitable conditions.

In the psychological clinic, real needs are brought to the psychologist, but without an appropriate approach the therapist cannot get conditions which satisfy the requirements of scientific methods, i.e., he cannot make predictions about behaviour which can be verified.

A clarification of a "here and now" psycho-analytic method would seem to constitute an important methodological advance in studying the phenomena of need-relationships because it opens an avenue for prediction and eventually for quantification.

It has been stated that only within the therapeutic relationship will the individual submit what matters in his life, the forces in his private world, to the examining microscope of the scientist. It is equally true that effective therapeutic relationships will be achieved only by the building-up of a corpus of scientific knowledge.

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XIX

PSYCHOLOGICAL RESEARCH IN THE FIELD OF NEUROLOGY

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INTRODUCTION

THE purpose of this paper is to provide a short account of some research studies at present in progress in the neurological field. The mere fact that these studies are being undertaken at all is a tribute to the many British neurologists and neurological surgeons who have given us facilities, encouragement and support. We deeply hope that the outcome of our work will not prove unworthy of the confidence that has been placed in us.

Before I begin my review, I should like to make some general remarks about psychological work in the neurological field. On one hand, it has many—and in my view important—advantages. I need only mention the unrivalled opportunity it presents for study of the effects of cerebral lesions upon human performance. In this respect, at least, it has advantages which for obvious reasons are denied to the laboratory worker. On the other hand clinical study, by its very nature, imposes limitations on the research worker unknown to his laboratory colleague. In the first place, it is seldom—if ever—possible to design an experiment with full control of the relevant variables. In the second place, there are few theories to give direction to research. Experimental testing of hypotheses in the neurological field has barely begun. In the third place it must never be forgotten that the

psychologist's first responsibility is to the individual patient. Research in the neurological field can be conducted only within the humane traditions of British medicine. None the less, I am convinced of the need for strict loyalty to the experimental method, of which Sir Frederic Bartlett has reminded us in a distinguished address to the Twelfth International Congress of Psychology held in Edinburgh in 1947. Although not all the work I shall describe to-day can properly be described as experimental, I am hopeful that you will not regard it as disloyal to basic scientific principles.

It is unnecessary to state that work of great importance to psychology is being carried out by the neurologists themselves in a variety of centres. As my subject is limited to the contribution of psychologists, however, it would not be proper to refer to it in the present context. I shall, therefore, confine my account to research in progress in three centres in which psychologists are active and in which I have some personal acquaintance with their work. These are, first, the Psychological Department of the National Hospital, Queen Square, under the supervision of Dr. Eliot Slater; second, the Department of Neurology and Head Injuries Bureau, Oxford, under the direction of Dr. W. Ritchie Russell; and third, the Nuffield Department of Surgery at the Radcliffe Infirmary, Oxford, under the direction of Professor Sir Hugh Cairns.

The general aim of our work has been to elucidate some of the relations between neurological disorder and changes in the psychological sphere. This work is proceeding in a number of directions and involves a concentrated attack on a number of the special problems presented by the clinical material. I have selected six separate lines of investigation for special mention. These are as follows:

1. A study of autonomic changes in situations of stress, with special reference to the effects of the operation known as prefrontal leucotomy. This work is being carried out at the National Hospital by Dr. A. Elithorn, of the Medical Research Council

Neurological Research Unit, in co-operation with Mr. M. F. Piercy, who is on the staff of the Psychological Department.

2. A study of disorders in visual space perception and related functions associated with lesions of the parietal lobes. This study is proceeding under my direction at the National Hospital and in Oxford.

3. An investigation of aphasia, handedness and cerebral dominance which is being carried out at Oxford by Mr. M. E. Humphrey under the direction of Dr. Ritchie Russell.

4. An investigation of organic amnesia by Mrs. Moyra Williams, working under the direction of Sir Hugh Cairns.

5. A study of defects in abstraction and kindred higher mental processes in cases of cerebral lesion. This work is being undertaken in the Psychological Department of the National Hospital. Some work along the same lines is also in progress in the Psychological Department of Barrow Hospital, Bristol, under the direction of Dr. K. R. L. Hall.

6. A study of cases of infantile hemiplegia treated by the surgical expedient of complete removal of the grossly diseased cerebral hemisphere. This operation is known as hemispherectomy. Dr. J. McFie has studied eleven cases before and after operation by Mr. Wylie McKissock at the National Hospital, while, in Oxford, Sir Hugh Cairns has placed a small number of cases for investigation in the capable hands of Miss M. A. Davidson, Clinical Psychologist to the Warneford Hospital.

A short account of our progress in these various fields of inquiry may be given.

AUTONOMIC CHANGES

The method devised by Elithorn and Piercy consists in studying the relationship between autonomic responses to a painful stimulus (an electric shock) and to a warning signal (a light) preceding the shock. Studies are being made in both healthy and disturbed subjects. The responses are recorded galvanometrically using the classical skin-resistance (P.G.R.) technique. Among the

first findings—incidental to the main problem—was the discovery that autonomic responses show "local sign", i.e., the response to a given stimulus is greater on the side stimulated than on the contralateral side of the body (Elithorn, Piercy and Crosskey, 1951). This is well shown in Fig. 17. So far as we are

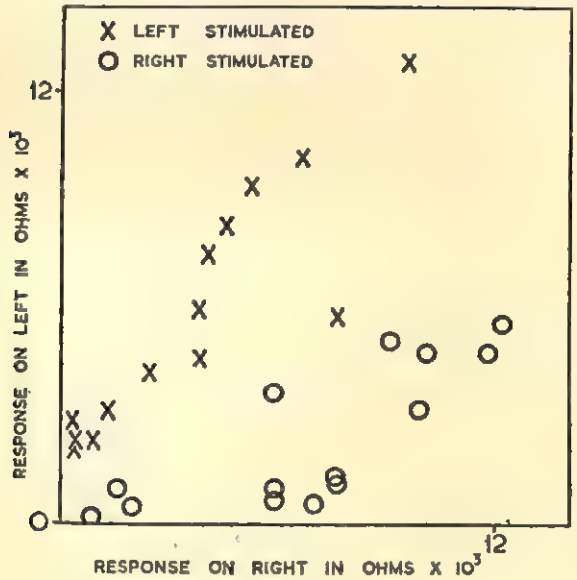

FIG. 17.—Local Sign in Autonomic Response

Graphic representation of local sign in skin resistance changes (method 1) following painful stimuli. The autonomic responses represented here are the raw data on subject M.A.C. The circuit on which each response is recorded is not shown here but the side stimulated is indicated by a symbol, and the size of the response on each side of the body is shown by the position of the symbol in relation to the co-ordinates

aware, this "local sign" in automic response has not previously been demonstrated in the intact human subject. As regards the effects of stress, the results to date suggest that psychoneurotic individuals, and some normal subjects in states of anxiety, show *a decrease in the ratio of response to shock to response to warning light*

signal with repetition of the experimental situation. In calm subjects, on the other hand, the reverse holds. After leucotomy, it is found that a patient's responses often approximate much more closely than before operation to those of an equable normal subject. A typical record is shown in Fig. 18. This finding may well prove important for an understanding of post-operative emotional change in anxiety states.

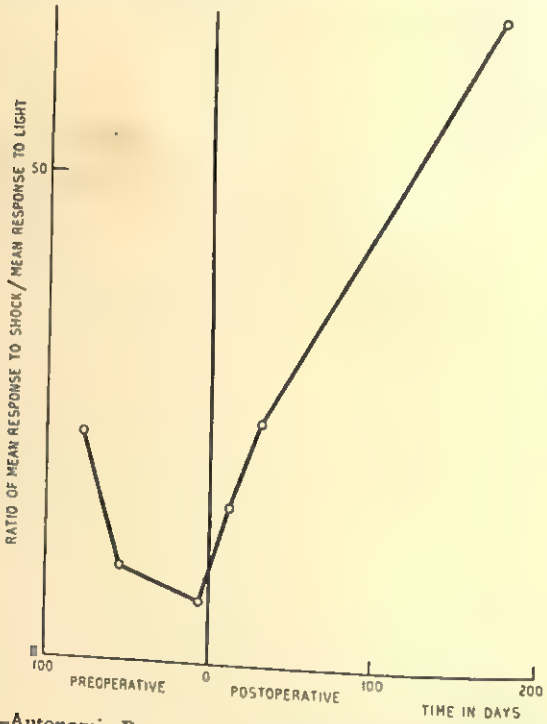

FIG. 18.—Autonomic Responses in a Patient before and after Pre-Frontal Leucotomy

PARIETAL LOBE SYNDROMES

We have undertaken during the last few years several studies of the effects of parietal lobe lesions on the organization of visual

space (Paterson and Zangwill, 1944, 1945; McFie, Piercy and Zangwill, 1951). In cases of bilateral lesion, as Gordon Holmes showed many years ago, visual space may be completely disorganized. In cases of unilateral lesion, on the other hand, the perception of space is at first sight unaffected. Depth and distance perception are as a rule intact and visual-motor co-ordination is not grossly affected. None the less, we have found that such

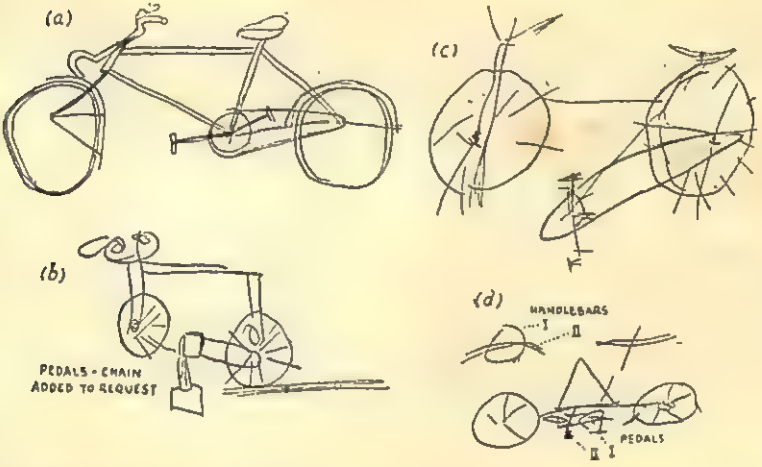

FIG. 19.—Disorganization in Drawing in Patients with Lesions of the Right Cerebral Hemisphere

cases show extreme defects in understanding spatial relationships and in fine manipulation under visual control. These defects are well illustrated in Figs. 19 and 20.

There are a number of points about these parietal syndromes which interest us. First, the question of memory for routes and places. A number of our patients have shown some impairment of topographical orientation and we have reason to believe that, in some of these cases at least, visual memory is at fault (Zangwill, 1951). At any rate, visualization has been disturbed in several of the patients and, curiously enough, they may entirely cease to

dream (Humphrey and Zangwill, 1951). We are therefore investigating the visual component in spatial orientation with particular care. Second, we have been much struck by the frequency with which visual-spatial disturbances have been associated with lesions of the minor cerebral hemisphere—the right in right-handed individuals. We are therefore making comparative studies of the effect of left and right-sided cerebral lesions upon higher visual functions. Third, we are trying to

FIG. 20.—Assembly of the "Manikin" Figure by a Patient with a Right Occipito-Parietal Lesion

study the possible bearing of our findings on the question of individual differences in manipulative ability among normal subjects. It is entirely possible that the neurological approach may throw new light on the basis and meaning of special abilities and disabilities in the healthy individual.

APHASIA AND CEREBRAL DOMINANCE

M. E. Humphrey is investigating the problem of aphasia in left-handed individuals with unilateral injuries of the brain. A preliminary study of the hand-preferences shown by seventy healthy left-handed subjects in a variety of everyday tasks made

it clear that lateral dominance in such individuals is seldom as clear-cut as in the right-handed (Humphrey, 1951). Indeed a certain "ambilateral" tendency is shown by many self-styled left-handers. This finding throws some doubt on the conventional view that the right hemisphere in left-handed people is necessarily the "dominant" hemisphere in respect of language processes. In consequence, Humphrey was led to work through the records of all left-hand patients with history of aphasia in the archives of the Head Injuries Bureau at Oxford. Preliminary results (Humphrey, 1951), suggest that aphasia in left-handers is liable to follow unilateral lesions of *either* hemisphere, though it may be more severe and long-lasting if the left hemisphere is affected. Out of ten cases selected for special study, five had had aphasia following a left-sided and five aphasia following a right-sided head injury. In general, the five patients with left-sided injuries sustained the more severe and lasting language disabilities. It was interesting to note that the latter were relatively more impaired on spelling whereas the cases with right-hemisphere lesions were relatively more impaired on calculation. These findings suggest that, in left-handed patients the relations between handedness and cerebral dominance are far less clear-cut than in comparable cases with lesions of the left cerebral hemisphere. The theoretical significance of this conclusion remains to be assessed.

Among other recent studies of aphasia, I would like to mention a communication from Oxford by Elvin and Oldfield (1951) on the disabilities and progress of a dysphasic University student. So far as I am aware, this is the first systematic study of the effects of dysphasia upon high-level intellectual performance and should set a model for future studies.

AMNESIC STATES

Our work on memory disorders is following two main lines. First, we are attempting to study the breakdown and restitution

of memory for recent events after concussional head-injuries. Second, we are interested in the relationship between more prolonged amnesic states and circumscribed lesions of the brain. As regards head-injuries, Moyra Williams has been assembling evidence which suggests that traumatic amnesia is seldom as well-defined as is commonly supposed. In some of her cases, well-marked disturbances in memory for events preceding the injury have been elicited which go beyond the period of retrograde amnesia as conventionally ascertained. These include memory-gaps, haziness in recollection of recent events, and disturbances in temporal organization (Williams and Zangwill, 1952). As regards amnesic syndromes, the available evidence suggests that diencephalic lesions are of particular significance in this connexion. In association with Sir Hugh Cairns, we have been able to study several cases of amnesia associated with lesions in the general neighbourhood of the third ventricle, which it is hoped to communicate at a later date. From the psychological point of view, the interest of these findings lies in the unsuspected relevance of the diencephalon to the problems of learning and memory—so long considered exclusively in terms of the functions of the cerebral cortex.

FIG. 21.—Regression in Korsakoff's Psychosis: The Drawing is intended to Represent the Latest Fashions in Dress

We have also made some studies of the prolonged amnesias sometimes found in patients with diffuse cerebral disease. In one study, we paid special attention to changes in generic imagery associated with extensive retrograde amnesia (Zangwill, 1950). The patient, who suffered from Korsakoff's psychosis, presented

a retrograde amnesia covering some fifteen years of her past life. Her drawings of people showed that her idea of fashion in dress had regressed accordingly (cf. Fig. 21). A similar regression was shown in drawings of other objects subject to progressive change in design—e.g., a motor bus. These findings suggest that amnesia may blot out the generic no less than the personal implications of past experience.

DEFECTS OF ABSTRACTION

I wish now to consider briefly some disorders of thinking associated with cerebral injury or disease. The background of our work in this field is Goldstein's distinction between "abstract" and "concrete" behaviour, and his thesis that behaviour of the former type is especially vulnerable to dissolution in cases of cerebral disturbance. The brain-injured patient, Goldstein maintains, fails fully to appreciate abstract issues and is apt to respond to all types of problem in a characteristically concrete fashion. At an elementary level, this defect of conceptual grasp can readily be displayed on any test which requires the patient to sort items according to some appropriate set of categories (Goldstein and Scheerer, 1941). One such test is that of Weigl, sometimes known as the "Colour-form" Sorting Test. The patient is confronted with twelve pieces in three shapes and four colours. He is requested to group the items in the appropriate categories. Although a very easy test for the normal adult and even for the normal child above the age of ten years, it is apt to give special difficulty to the organic patient and in certain cases of schizophrenia.

We have results on the Weigl test from seventy-four cases with unilateral cerebral lesions examined in the Psychological Department at the National Hospital (McFie and Piercy, 1952). These results indicate that "categorical" defects are much more frequently found in cases with lesions of the dominant cerebral hemisphere. Roughly speaking, about half the cases with left-

sided (dominant hemisphere) lesions fail on the test whereas a comparable failure is found in under 5 per cent of the cases with lesions limited to the right hemisphere. The difference between the means for the two groups was found to be significant at the $p = \cdot 01$ level.

The presumptive importance of the dominant hemisphere for conceptual thinking emerges also from an analysis of the results given on a verbal abstraction test. On the Wechsler-Bellevue Similarities Test, we have found the same clear-cut superiority of the group of patients with lesions of the right (non-dominant) side. Further, we have additional evidence to the effect that the inferiority of left-sided patients is not wholly—or even predominantly—due to the presence of aphasia in certain of the cases in this group.

The relationship between abstraction defects encountered in organic psychoses and in the schizophrenic thought disorders has been recently studied by Hall (1951). His work suggests that, in some cases of schizophrenia at least, there is an intellectual deficit surprisingly similar to the organic type of abstraction defect.

HEMISPHERECTOMY

McFie has studied eleven cases of hemispherectomy before and after operation, using standard psychometric tests appropriate to the mental and chronological age of the patients. Chronological ages varied between four and twenty years and mental ages between idiot and average adult levels. I.Q.s obtained before and at various intervals after operation are shown in Fig. 22. Whereas the upper levels represent genuine I.Q.s obtained on the Wechsler-Bellevue Scale, the lower ones represent developmental levels on Gesell's Scale and the intermediate ones Stanford-Binet values. In spite of this wide range of tests, however, the figures can serve as reliable standards for post-operative comparison.

It will be noted that (with one exception—Case 10) the level of ability does not fall after removal of one cerebral hemisphere. In the cases in which there was a post-operative rise, on the other

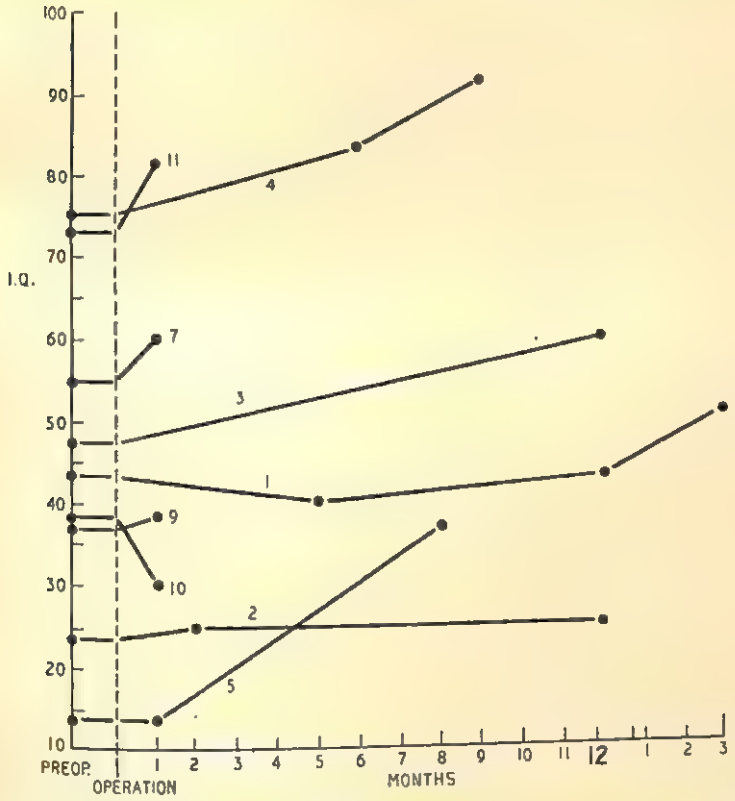

FIG. 22.—Intelligence Quotients in Cases of Hemispherectomy before and after Operation

hand, it cannot necessarily be concluded that ability has improved. Quite distinct improvements on retesting are indeed to be anticipated. None the less, the rise in certain cases (e.g., Cases 5 and 11) is beyond that which would be anticipated on the basis of

practice effect. In these cases, too, apparent intellectual gain was correlated with psychiatric improvement.

An interesting feature of these results was that, on pre-operative testing, verbal abilities were found to be inferior to non-verbal abilities. This applied in all cases irrespective of which hemisphere was damaged. After operation, although the scores rose, this discrepancy remained. One may add that this superiority of non-verbal over verbal ability is the reverse of what is usually found in cases of brain damage.

These findings are of considerable interest. It appears that removal of an entire cerebral hemisphere had no significant adverse effect on the intellectual level of these young patients. If anything, indeed, they show improvement. It must, of course, be borne in mind that the hemisphere removed had been grossly diseased for many years, in some cases from birth, and it would be wholly unfair to draw inferences from the findings regarding the presumptive effects of removing an entire hemisphere from a healthy individual. None the less, the results cause one to review one's ideas regarding the intellectual functions of the cerebral cortex in man.

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THE TEACHING OF PSYCHOLOGY

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PREVIOUS DISCUSSIONS

THE teaching of psychology, like most human enterprises, has developed in rather a random way. We cannot speak of plans, but of opportunities, accidents, and personalities. For the past ten years or so many people have been unhappy about this state of affairs. It has seemed to them that psychologists are coming to play so big a part in our society that a training at once more carefully planned and more standardized must be provided. Reflecting this trend of opinion there have been numerous papers and reports, particularly since 1947, in which year there appeared Dr. Alan Gregg's report on "The Place of Psychology in an Ideal University", as well as a symposium on "Training in Clinical Psychology", both from the United States, and a survey by Professor Révész of Amsterdam entitled "Die Bedeutung der Psychologie". In this country two committees have reported to the British Psychological Society, one on undergraduate the other on graduate training. There have been in addition several symposia, and indeed the field, far from being unexplored, is beginning to show something of the trampled muddiness that one finds in a rugger pitch on a wet Saturday afternoon. All that I can attempt in this paper is an outline of some of the points on which a fair measure of agreement has been reached.

ESTABLISHMENT OF UNIVERSITY DEGREES IN PSYCHOLOGY

But before we consider where we are going it might be as well to look briefly at where we have been. When we consider the teaching of psychology, 1879 does not seem to have meant so much as we once thought. It may be that the founding of Wundt's laboratory marks an episode rather than a fundamental change in direction. It is true that the label "psychologist" was not widely used before his time but that does not mean there was no psychology. "Let us call the roll" says Boring in the chapter in his history which treats of Alexander Bain. "Descartes, philosopher and physiologist; Leibnitz and Locke, philosophers and men of political affairs; Berkeley, philosopher, bishop and educationalist; Hume, philosopher, historian and politician; Hartley, learned physician; James Mill, historian and diplomatist; John Stuart Mill, philosopher, logician and political economist; Charles Bell, Flourens, Johannes Müller, E. H. Weber, physiologists all". Other names could have been included. Some of these men taught at the Universities, all of them provided material for university teaching; and a good deal of that material was psychological. Wundt himself and William James worked through psychology from physiology into philosophy. Pavlov and Freud began as physiologists. Ward and Stout remained philosophers. Psychology was first a region with very permeable boundaries, with which a number of other fields seemed to overlap.

Once this region had been identified by a name its boundaries became important, and definitions of psychology during its first few decades of official life were embarrassingly numerous. It took its place during this period as a "subject", that is an academic discipline and a field for research with its own particular problems and methods. As such it was something which a student could "take" for a degree. Since the war degree courses in psychology,

which previously were few in number, have been established in most British Universities.

THE NEED FOR PROFESSIONAL TRAINING

In the meantime, however, people who had specialized in the new subject were found to have skills which made them useful in the educational, the industrial, and more recently the clinical fields. In America to-day the largest single group of psychologists is that engaged in applied work of one kind or another. It is probable that a similar state of affairs exists in this country. As a result there has come into being something very like a profession —a group of people, namely, with roughly the same kind of training, doing work in the community under the label "psychologist". These people, have, within quite recent years, formed themselves into professional organizations. The work they undertake is very varied. Some help teachers to deal with difficult children, or give advice on placing within the educational system as a whole. Others assist psychiatrists on questions of diagnosis and research; still others are concerned with the wide range of human problems presented by modern industry.

Now an Arts or Science degree is not intended as a professional training. It is a systematic introduction to some particular subject or group of subjects. Professional training is recognizably different, involving as it does, some form of apprenticeship, some opportunity to carry out under supervision the operations which the profession will later require. In this connexion one thinks particularly of the long years of clinical training which the doctor must undergo. It seems reasonable to claim that if there is to be a profession of psychology there should be professional training for psychologists. This is not, of course, a mere argument from analogy. Every practising psychologist has probably been impressed by the gap between his initial training and what has been expected of him in the field.

We are in fact confronted with another accident of history, or

perhaps an opportunity. This time, however, it is not a matter of philosophers and physiologists or the re-shuffling of university subjects. The situation is a new one, and the initiative must come from the profession, or rather through the profession from the community at large. It will be evident from what has already been said about reports and committees that the profession is taking its responsibilities in this matter quite seriously. It is to be hoped that the universities will recognize the new situation when it is presented to them.

Certain points seem already to have been widely accepted. The first degree, it is felt, should not be used for professional training purposes. What is needed is something more rather than something different. Not that the first degree itself cannot be improved. We know well enough that merely talking to students is not the best way to teach them something. Experiments in teaching technique are being tried in many places, some of them such as Oeser's "project" technique, involving considerable departure from normal practice. But with all that the first degree in psychology remains strictly comparable to degrees in other academic subjects.

FACILITIES FOR POST-GRADUATE TRAINING

In this country, however, the restriction of professional training to the graduate level presents a number of difficulties. In the United States the additional teaching commitments have been readily undertaken because large graduate departments were already in existence. Here, in the main, graduate work tends to involve research only, and there is not a great deal of formal teaching. Exceptions can be found. An example near at hand is the Scottish Bachelor of Education, perversely named since most of those taking it must already have a master's degree. But by and large the staff and accommodation in British universities tends to be planned for undergraduate teaching. Graduates, it is felt, can look after themselves.

More important even than staff and space is the question of facilities. You cannot train professional psychologists in the classroom and laboratory. Consider the young graduate as we shall know him when the supply of ex-service students has dried up. He will have left school at eighteen. Then or at the end of his first degree he will have put in his period of national service. No amount of additional lectures or vicarious experience will enable him to collaborate on cases with psychiatrists or give worth-while advice to teachers and industrialists. He must learn on the job, and yet learn in such a way that his mistakes will damage neither his clients nor his confidence.

Apprenticeships in the field are almost non-existent. There are far too few psychologists there, and most of those are unable to add training to their existing commitments. It seems that the only solution is to have in university departments, or linked with them, real psychological work of the kinds in which training is required. This is no more than the principle of the teaching hospital and has been widely accepted. Most psychology departments already handle consultant and similar work, but at present many of them do it unofficially as a "hobby" to use Professor Wolters's word. But since professional training will presumably involve the granting of diplomas or higher degrees, perhaps the next step is to have the whole thing discussed within the universities and, if possible, put on an official basis. Without a considerable increase in existing facilities there seems no way of getting rid of the present awkward bottleneck between the degree and the job. But there are dangers in all this and we must try and be clear about them.

DANGERS IN TOO MUCH PROFESSIONALISM

Psychology, as a theoretical and experimental enquiry, occupies an area somewhere between neuro-physiology on the one hand and social anthropology on the other. There are stresses within the area, and we see at Harvard how the psychological

field can be split into experimental laboratory work on the one hand and field studies on the other. Were psychologists to become too much preoccupied with practical and professional matters it might well happen that this theoretical gap would widen and each side of it become assimilated to the adjacent science. What we now call the science of psychology might become either neuro-physiology or social anthropology. If that were to happen then professional psychology, for all its tests, its techniques and its other developing skills would become what a psychiatrist has called psychiatry—the application of a science which does not yet exist. All these new preoccupations must not be allowed to distract psychologists from their main task which is the scientific and even, may one add, the experimental study of human behaviour.

Then again the old link between psychology and philosophy may seem at first sight of little importance when we think of practical matters. There could be no greater error. As a scientist the psychologist shares with other scientists a need for logical criticism and clarification, but as a practitioner he needs much more. His work forces him to make value judgements of a very far-reaching kind. Doctors have to do this too, but their task is easier. Health after all is clearly better than sickness. Psychiatrists find it less easy. Is a depressed patient with an over-developed conscience better or worse than a carefree libertine? Psychologists, above all, since they are dealing with the problems of more or less normal people have to be able to satisfy themselves that the changes which they seek to bring about do in fact constitute changes for the better. Some of the issues raised here demand the widest possible scrutiny of the human situation. Unless we can deal with issues of this kind we shall only be used as technicians, and that is what we shall deserve.

There is an even wider question in which value judgements are involved. Lionel Trilling, in his essay on the Kinsey Report writes: "There is something repulsive in the idea of men being studied for their own good. The paradigm of what repels us is

to be found in the common situation of the child who is *understood* by its parents, hemmed in, anticipated and lovingly circumscribed, thoroughly taped, finding it easier and easier to conform internally and in the future to the parents' own interpretation of the acts of the past, and so, yielding to understanding as never to coercion, does not develop the mystery and wildness of spirit which it is still our grace to believe is the mark of full humanness. The act of understanding becomes an act of control." This is very plausible; perhaps it is true. At any rate, one would not wish the psychologist to become so confined and biased by his professional training that he could not entertain the possibility that the world might be a better place without him.

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